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TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS.

REGULAR MEETING, NEW YORK, OCTOBER 25TH, 1892.

Electric Railways.

JOINT DISCUSSION OF THE FOLLOWING PAPERS, READ JUNE 8TH:

SERIES ELECTRIC TRACTION, by Nelson W. Perry.

A NEW SYSTEM OF ELECTRIC PROPULSION, by H. Ward Leonard.

ELECTRIC RAILWAY MOTOR TESTS, by Prof. Geo. D. Shepardson and Edward P. Burch.

PUBLISHED MONTHLY BY THE INSTITUTE, UNDER THE SUPERVISION OF THE EDITING COMMITTEE.

RALPH W. POPE, *Secretary*,

12 WEST THIRTY-FIRST STREET.

NEW YORK CITY.

PAGE

$$2\,W = 4\int^{B} \rho_0\, B\, d\, B = 2\,\rho_0\, B^2 \qquad (3)$$

as available energy.

The lost energy being $H = \eta\, B^{1.6}$, at the point

$$H = 4\,W, \qquad (4)$$

the law of 1.6th power must have ceased to exist.

From (4) we derive

$$2\,\rho_0\, B^2 = \eta\, B^{1.6}, \quad \text{or}$$

$$B_0 = \left(\frac{\eta}{2\,\rho_0}\right)^{2.5}, \qquad (5)$$

as the lowest limit of the law of hysteresis.

Substituting in equation (5) the values for the sheet-iron in Chapter I.

$$\eta = .0035 \qquad \rho_0 = .00343,$$

we derive

$$\left.\begin{aligned} B_0 &= .19 \\ F_0 &= .00067 \end{aligned}\right\}. \qquad (6)$$

Such low values, equal to $\frac{1}{1\,6\,0}$ the intensity of the magnetic field of the earth, are still rather out of reach for experimental research, the magnetic potential representing there only .00006 ergs.

Yonkers, N. Y., October, 1892.

TABLE OF 1.6TH POWERS.

B in 10^3. $B^{1.6}$ in 10^6.

B	.0	.1	.2	.3	.4	.5	.6	.7	.8	.9	Δ
	$B^{1.6}$ =										
0	.0	.00139	.00480	.00919	.01457	.02082	.02787	.03566	.04416	.05330	9—20
1	.06310	.073	.084	.096	.108	.121	.134	.148	.162	.176	10—13
2	.1913	.208	.222	.238	.255	.272	.290	.308	.327	.346	15—20
3	.366	.386	.406	.427	.448	.469	.491	.513	.535	.557	20—23
4	.580	.603	.627	.651	.675	.700	.725	.750	.776	.802	23—26
5	.828	.855	.882	.909	.937	.965	.993	1.022	1.051	1.080	27—29
6	1.109	1.139	1.169	1.199	1.230	1.261	1.292	1.324	1.356	1.388	30—32
7	1.420	1.453	1.486	1.519	1.552	1.586	1.620	1.654	1.688	1.723	33—35
8	1.758	1.793	1.829	1.865	1.901	1.937	1.974	2.011	2.048	2.085	35—37
9	2.122	2.160	2.198	2.236	2.274	2.313	2.352	2.392	2.432	2.472	38—40
10	2.512	2.552	2.593	2.634	2.675	2.716	2.758	2.800	2.842	2.884	40—42
11	2.926	2.969	3.012	3.055	3.098	3.142	3.186	3.230	3.274	3.318	43—45
12	3.363	3.408	3.453	3.498	3.544	3.590	3.636	3.682	3.728	3.775	45—47
13	3.822	3.869	3.916	3.964	4.012	4.060	4.108	4.156	4.205	4.254	47—49
14	4.303	4.352	4.401	4.451	4.501	4.551	4.601	4.652	4.703	4.754	49—51
15	4.805	4.856	4.908	4.960	5.012	5.064	5.116	5.169	5.222	5.275	51—53
16	5.328	5.381	5.435	5.489	5.543	5.597	5.651	5.706	5.761	5.816	53—55
17	5.871	5.926	5.982	6.038	6.094	6.150	6.206	6.263	6.320	6.377	55—57
18	6.434	6.491	6.548	6.606	6.664	6.722	6.780	6.838	6.897	6.956	57—59
19	7.015	7.074	7.133	7.192	7.252	7.312	7.372	7.432	7.492	7.552	59—61
20	7.613										

Correspondence.

Dr. Chas. E. Emery :—It is not desirable after the author of a paper has closed a discussion to reopen it by merely reiterating any points previously brought out. In this case, however, the author, who has been pleased to call my remarks critical, has certainly answered them quite critically, and has taken particular pains to treat as mistakes every criticism, except perhaps one. It therefore becomes necessary to respond to the reasons which have been given, and I thereby expect to show that the original criticisms are fully sustained. The author's statement that he has made many calculations of the loss by hysteresis and found it less than one per cent., is directly responsive, but his suggestion that therefore I possibly have neglected to divide the revolutions per minute by 60, in order to obtain the number of complete cycles per second, and therefore obtained an erroneous result, while it may to our auditors be considered a good joke, is rather one-sided. No such mistake has been made. The difference in result may undoubtedly be explained by the fact that the loss is stated "in watts per cubic centimetre," which is evidently independent of the output or the number of watts in the main circuit. By substituting the particular values given in the first discussion in the equation of Mr. Steinmetz, the loss will be found to be 0.031 watts per cubic centimetre.[1] This loss would occur in the armature core of a dynamo revolving in a magnetic field under conditions stated, when no current was circulating through the armature coils and when, therefore, there was no output to compare with the loss; but in the case of a motor with separately excited field and a very small current through armature, the whole work might be absorbed in friction and hysteresis, so that the proportional loss due to the latter would be very large. Evidently, in any case, the

1. A calculation based on the formulæ of Mr. Steinmetz, for the conditions originally stated, is herewith presented.

1st Eq., p. 685. $W = \eta N 10^{-7} \left(\frac{L_1 - L_2}{2}\right)^{1.6}$

Bottom p. 685. W = watts lost per cubic centimetre.

Top p. 685, N = number complete periods per second.

$L_1 - L_2$ = maximum values of magnetic induction in lines per cm.^{2} (bottom of page 685; page 658 and elsewhere the same characters refer to kilolines).

From first line referring to sheet-iron, Table XLVII., page 680, $\eta = 0.0035$.

Therefore, to find loss by above formula in the core of an armature making 1,000 revolutions per minute, with the magnetization alternating between minus and plus 16,000 lines per square cm., we have $L_1 = 16{,}000$, $L_2 = -16{,}000$; hence,

$$(L_1 - L_2) \div 2 = 16{,}000,$$

$$N = 1{,}000 \div 60 = 16.67 \text{ ; so}$$

$$W = 0.0035 \times 10^{-7} \times 16.67 \times (16{,}000)^{1.6} = 0.031.$$

proportional loss would decrease as the output was increased, and the percentage merely depend upon the relative output of the particular machine for which the comparison was made. It should be borne in mind that I do not attack Mr. Steinmetz's admirable discovery that the loss in induction due to hysteresis is proportional to the 1.6th power, etc. I simply point out that, by analogy, hysteresis only increases the reluctance and, like the increased reluctance due to an air-gap, the loss of energy should be measured only by the necessary increase of exciting force.

2. Mr. Steinmetz states, next:—"Here I must correct a mistake also. The hysteresis and eddy-current losses in dynamos do *not* enter into the electrical efficiency at all, but only into the mechanical efficiency, there showing as an apparent increase of the mechanical friction." The latter statement is of course correct, but the impropriety of founding the first conclusion on a mere "apparent increase" becomes evident in distributing the electrical energy delivered to a motor. The mechanical friction is easily separated by a transmitting dynamometer and may be added to the exterior mechanical work performed when motor is loaded, as shown by any form of dynamometer, but when the total mechanical work, including friction, is subtracted from that due to the watts in the circuit there is, even when the volts lost by resistance are considered, a residual loss, or a certain proportion of the electrical energy unaccounted for, and this only is available for application to hysteretic, eddy-current and other electrical or electro-magnetic losses. The same distribution should be made in the case of a dynamo. It is certainly wrong to call everything a mechanical loss that merely appears to be so, because the friction of the apparatus is not commonly separated.

3. The preliminary remarks of Mr. Steinmetz about transformers are interesting, but again avoid the true question, which is not to dispute the facts about hysteretic losses, but to ascertain whether the equations, for given conditions, show simply the loss of induction or the direct loss of energy.

4. The next statement by Mr. Steinmetz, that it has been proved by calculation that an amount of energy disappears out of the M. M. F. equal to the area of the hysteretic loop, is subject of course to the uncertainty of calculations on such a subject, and the statement is apparently directly in conflict with No. 3 above, where he claims that the hysteretic loss does not affect the electrical efficiency.

5. My reference to "time lag" was merely an illustration, not a definition, and the elaboration of Mr. Steinmetz on the latter basis only makes the former more apparent.

6. Mr. Steinmetz calls attention to the fact that no such thing as "extra turns" exists in transformers, and by showing that the ratios of transformation depend upon the relative number of turns in the primary and secondary, argues therefrom that my suggestion is incorrect, viz: that the loss by hysteresis should be

measured by the extra exciting force necessary to overcome the increased reluctance, instead of being measured by the decrease of magnetization. Evidently, however as both primary and secondary operate in connection with the same iron, any increase in the reluctance thereof must affect both primary and secondary alike, and not change the ratio of transformation, but simply reduce the efficiency of the apparatus as a whole, as is found in practice. Either circuit may evidently be the primary or the secondary, and technically the extra turns to overcome reluctances of all kinds are found in both.

7. Mr. Steinmetz kindly calls my attention to the fact that I have improperly given him the credit, which belongs to Mr. Kennelly, of first applying a reluctivity formula derived from Frölich's function to the *metallic* induction. On again consulting Mr. Kennelly's paper, I find that Mr. Steinmetz is right as to the credit, and I cheerfully make the correction, but that I am right in my illustration and Mr. S. is wrong in his conclusion relating thereto. Mr. Kennelly did make his first application to the total induction, as I supposed, and later developed the application to the metallic induction. Fig. 3, in my original paper, was based on an example given in the first part of Mr. Kennelly's paper, where the reluctances are the actual reciprocals of the permeabilities;[1] so that my illustration as to the inapplicability of the Frölich function in that particular case is correct, and for the low magnetizations shown, a consideration of the metallic induction would not have changed the results. The further discussion of Mr. Steinmetz as to the applicability of this function, is somewhat a repetition of the original discussion, but will be reverted to later.

There is a chance for a difference of opinion in regard to the use of the symbol I, and in any case the matter is of no further consequence than I have already stated.

Mr. Steinmetz states in substance, that the particular experiments on wrought-iron which I refer to in his paper, were not the average of experiments from five samples, nor a fair average of all the experiments. I find that the experiments selected were from but one sample, but that such experiments were those also specially selected by Mr. Steinmetz himself, and placed in a separate table near the beginning of his paper (page 626) as if representing a typical kind of wrought-iron, while the results of other experiments are given in the large table at page 680. I do, however, find on page 692 a statement of the "Magnetic Properties of Average Materials," in which the value for wrought-iron for $F = 60$, $B = 16{,}700$, or 65 lines higher than for the sample tabulated at page 626, but this value still shows that the corresponding value, viz., $B = 18{,}650$, given in the first paper, was much too high, so my criticism is sustained. This part of the criticism is,

1. TRANSACTIONS, vol. viii., p. 504.

however, finally acknowledged in general terms by Mr. Steinmetz after a tedious reference to details, as he explains in substance that the higher magnetizations given in his first paper for wrought-iron plates were different because determined by the electro-dynamometer method. This is satisfactory. I, however, also stated in my first discussion that the higher magnetizations shown by the *later* experiments are too *low* compared with the results reported by other observers. Differences of material will account for some variations, of course, but not for those practically changing the shape of the curves. The result may possibly be partly explained by the fact that Mr. Steinmetz used cores built up of rectangular plates to form a complete magnetic circuit on the log-house principle of piling, so that there was necessarily some reluctance at the joints, and it is gratifying to know that such a construction produces no more reluctance. It seems proper that all these differences should be pointed out and with the coincidences, clearly stated.

As the electro-dynamometer apparatus offers facilities for multiplying experiments to a degree not possible with any other method, we cannot too highly appreciate its value so long as the fact is known as was hinted in my first discussion that the instrument gives relative, but not absolute, results through all ranges. The latest discovery of Mr. Steinmetz, that the hysteretic loss as he terms it is proportional to the amplitude and not to the absolute values of the changes in magnetization, is one entirely of comparison and its confirmation by two methods entirely satisfactory; but the evidence is by no means clear, that in a general sense the elements of curves of magnetization may be derived from a linear reluctance formula. Most of the experiments which seem to prove the law through wide ranges, appear to have been made with the electro dynamometer, though both Mr. Kennelly and Mr. Steinmetz give many others that approximately follow the law for short ranges. It is a pleasure to acknowledge that the function as applied, furnishes good approximate formulæ, but from the present evidence we must consider such formulæ empirical rather than rational. I am gratified to say that Mr. Kennelly, though he first pointed out the coincidences, was more cautious in assuming the law general than Mr. Steinmetz has been.

Mr. Steinmetz:—Referring to Dr. Emery's communication, I may add a few remarks:—Leaving aside all the metaphysical speculation, which I consider as of rather little interest, and everything which calls for a simple repetition of former remarks, I intend to deal only with some misunderstandings. For instance, I did not say that the hysteretic loss in the dynamo machine is a mechanical loss. It is neither a *mechanical* nor an *electric*, but a *magnetic* loss. What I said was, that this hysteretic loss enters as term into the *mechanical* efficiency only, but not into the *elec-*

trical efficiency. As well known, the *electrical* efficiency is the ratio:

$$\frac{\text{whole electric energy} - \text{electric losses}}{\text{whole electric energy,}}$$

while the *mechanical* efficiency is the ratio:

$$\frac{\text{whole supplied energy} - \text{whole losses}}{\text{whole supplied energy.}}$$

That in the electric motor, the magnetic just as well as the frictional or any other loss of energy is in the end derived from electric energy, is self-evident. But even in the electric motor the energy lost by molecular magnetic friction is not directly derived from the *electrical* energy supplied, but from *mechanical* energy, which in turn is produced by electrical energy. Consequently, in calculating the losses in electric motors, the calculated loss by hysteresis has to be increased by the coefficient of loss of the electro-mechanical conversion of energy, to get the corresponding expenditure of electric energy. In alternating apparatus, however, the hysteretic losses are generally directly derived from electrical energy

With regard to the 13 per cent. of hysteretic loss given by Dr. Emery, I could not suppose indeed that he gave the percentage of loss *for a dynamo running light*. At least the 90 per cent. efficiency mentioned by him did not point that way.

With regard to Dr. Emery's belief in the uncertainty of Ewing's and Warburg's calculations of the meaning of the area of the hysteretic loop, I can refer to Ewing (p. 99), and Warburg (Wiedemann's Annalen, 1881, p. 141). But Dr. Ewing's remark that "I claim that the hysteretic loss in transformers does not affect the electrical efficiency," is just the opposite from what I did, for I gave the percentage of power lost by hysteresis for a number of types of transformers.

Referring to the "increase of reluctance," which consumes the hysteretic loss according to Dr. Emery; first, molecular friction produces no increase of reluctance; and second, since reluctance does not consume energy, neither could its increase consume energy, and if the problem of constant secondary potential, and the reac ion of the "leakage current" of the transformer upon the generator would not require a very low reluctance, the reluctance of the transformer may be very much increased without any decrease of efficiency, as it is done in the hedgehog transformer of Swinburne.

The agreement or disagreement of the "metallic induction" in Kennelly's paper (p. 504), with Fröhlich's formula, is a matter of opinion as to what constitutes a decided disagreement, and what is within the errors of observation. Dr. Emery's comparative curves in his paper "On Magnetization" (TRANS., vol. viii., p. 206), look unfavorably indeed, showing at some points differences of

three to four per cent. To decide the question, I may give here the readings from Kennelly (p. 504), with the values of ρ, calculated by the formula $\rho_0 = .1 + .058\ H$,[1] given by Kennelly, and the differences between observed and calculated reluctivities.

TABLE LXI.

ROWLAND WROUGHT-IRON RING.

H	ρ obs.	ρ calc.	Δ	$= \%$
2.877	.272	.267	−.005	−1.8
5.52	.412	.420	+.008	+1.9
7.426	.522	.531	+.009	+1.7
9.894	.664	.674	+.010	+1.5
17.30	1.104	1.103	−.001	− .1
33.72	2.083	2.056	−.027	−1.3
46.32	2.747	2.787	+.040	+1.4
Average			±.014	±1.4

The maximum difference between observed and calculated values is 1.9 per cent. The differences are irregular, but show the behavior generally noticeable on ballistic test by the step-by-step method, to lie alternately below and beyond the curve, which I ascribe to the cumulation of the errors introduced by the "sluggishness" of the iron For a classic sample of this tendency see Kennelly, p. 498. [TRANSACTIONS, vol. viii.]

Dr. Emery's opinion that most of the tests which seem to prove the linear law of reluctivity through very wide ranges, appear to have been made by the electro-dynamometer, can hardly be upheld, for of all the numerous tables collected in Mr. Kennelly's paper, *not one* has been derived by the electro-dynamometer method, but all by the ballistic, or by the magnetometer method. My experience with this instrument was just the opposite of Dr. Emery's opinion of it, for I found the electro-dynamometer very suitable—and perhaps the best instrument—to get *absolute values* of reluctivity within a *limited* range, but *entirely unsuitable* to give *relative results through all ranges*, so that for the latter purpose I was obliged to discard it altogether. Electro-dynamometer readings agree with the linear law of reluctivity only within a limited range, so that this law can not be due to this particular method.

With regard to the air reluctance introduced in my electro-dynamometer tests by building the magnetic circuit up of rectangular pieces of iron, I certainly have not forgotten to calculate the reluctance of these air-gaps and to determine the influence, but

1. The value of σ, of this sample tested by Rowland, and consequently the value of absolute saturation $I_\infty = 17.24$ is accidentally just the same as that of the sheet iron in Table I. of my paper, which Dr. Emery considered so low a value as to suspect a mistake. Ewing indeed found, in his "very soft iron wire," a still much lower value, $I_\infty = 15.75$.

TABLE LXII.

IRON, COBALT AND NICKEL IN STRONG MAGNETIC FIELDS.

(Dubois, Ewing, p. 158.)

F	Soft Swedish Iron, Annealed.					Nickel at 100° C.					Cobalt at 100° C.				
F	L	ρ obs.	ρ calc.	Δ	$= \%$	L	ρ obs.	ρ calc.	Δ	$= \%$	L	ρ obs.	ρ calc.	Δ	$= \%$
80	17.70	4.5	4.7	+ .2	+4.3	3.92	20	20	.....	.....					
160	19.10	8.4	8.4	.....	.....	4.70	34	34	.....	.....					
240	19.8	12.1	12.0	− .1	− .8	5.08	47	47	.....	.....	10.7	15.0	15.0		
320	20.4	15.7	15.6	− .1	− .6	5.37	60	59	−1	−2	11.65	20.7	20.5	−.2	−1.0
400	20.8	19.3	19.3	.....	.....	5.52	72	72	.....	.....	12.3	26.0	26.0		
480	21.1	22.8	22.9	+ .1	+ .4	5.63	85	85	.....	.....	12.8	31.3	31.5	+.2	+ .6
560	21.2	26.5	26.5	.....	.....	5.71	98	98	.....	.....	13.0	36.9	37.0	+.1	+ .3
640	21.3	30.0	30.1	+ .1	+ .3	5.75	112	111	−1	−1	13.2	42.4	42.5	+.1	+ .2
800	21.4	37.4	37.4	.....	.....	5.85	137	137	.....	.....	13.3	48.1	48.0	−.1	− .2
960	21.45	44.8	44.6	− .2	− .5	5.90	163	163	.....	.....	13.55	59.0	59.0		
											13.7	70.1	70.1		
	Average.....			± .08	± .7	Average			± .2	± .3	Average			±.08	± .26
	$\rho = 1.13 + .0453\ F.$					$\rho = 7.6 + .162\ F.$					$\rho = 4.0 + .0688\ F.$				
	$L_\infty = 22.1.$					$L_\infty = 6.17.$					$L_\infty = 14.5.$				

found it negligible, which is explained by the fact that the cross-section of each air-gap is 12 cm.², that of the sheet-iron piece only .11 cm.², so that the joint reluctances of the four breaks represent approximately an increase of reluctivity of $\rho = .0014$ milli-units per cm. length of the magnetic circuit, which has been subtracted from the readings.

Since the discussion has shifted to this question of the exactness of the empirical linear law of reluctivity, it may be of interest to give some tests made by DuBois on iron, nickel and cobalt, since these tests cover an enormous range of M. M. F., and just the critical range, between $F = 80$ and $F = 960$, where a deviation from the linear law, if existing, must be expected. For beyond $F = 1000$, absolute saturation is practically reached, and is:

$$B = H \times L_{\infty}.$$

These tests were made by the magnetometer method, on prolate ellipsoids (Ewing, p. 158), and gave the results shown in Table LXII.

A range from $F = 80$ to $F = 960$ can hardly be called short.

I do not wish to be understood, however, as claiming the linear law of reluctivity for all magnetic circuits. While proved for homogeneous materials, it is well known not to apply to complex magnetic circuits, as, for instance, the characteristic of the dynamo machine. A discussion of this case, of the *metallic reluctivity of heterogeneous materials*, is given in the annexed Appendix.

APPENDIX III.

3. *Limits of the Linear Law of Metallic Reluctivity.*—Within the range of the tests communicated in the foregoing paper we have seen the metallic reluctivity $\rho = \frac{F}{L}$ follow—beyond a certain minimum value of M. M. F.—the linear law,

$$\rho = a + \sigma F.$$

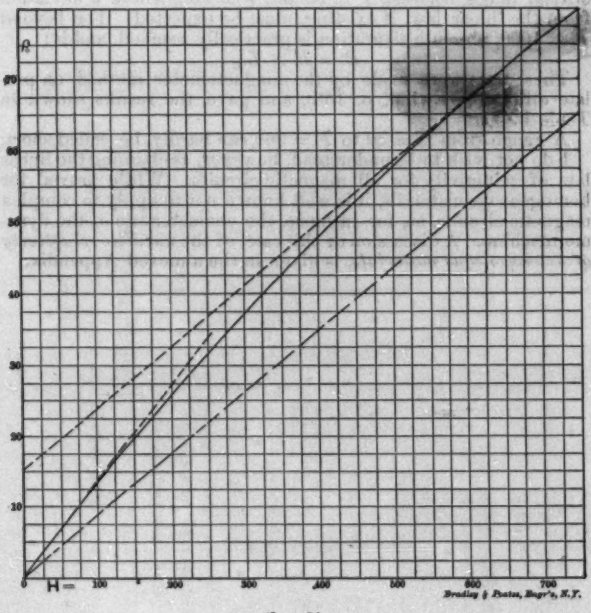

FIG. 31.

Below this minimum value of M. M. F. the metallic reluctivity of the alternating and of the ascending branch of the magnetic characteristic rise above, the reluctivity of the descending branch drops below the straight line represented by $\rho = a + \sigma F$, apparently due to the phenomenon of energy consumption by molecular friction.

Since Kennelly has shown that this linear law holds good up to the highest values of magnetization ever reached by Ewing in iron, steel, nickel and cobalt, and I found it proven also by my tests, this linear law of reluctivity seems to be established beyond doubt for all *homogeneous materials*.

Of *heterogeneous materials*, the only tests I know of are those of iron filings, on pages 702 to 710 of my paper. They agree with the linear law of reluctivity also, for the limited range of tests, but the remarkable fact is that in one and the same material the tests point to very different values of saturation, according to the speed of cycle. Besides, while in iron, steel, etc., by Ewing, absolute magnetic saturation has practically been reached; in these iron filings the highest readings are still far below the saturation limit. If, consequently, the linear law of metallic reluctivity ceases to hold anywhere, we can expect this only for heterogeneous materials.

Subjecting the case of such heterogeneous materials, composed of magnetic material of the equation $\rho = a + \sigma F$, and of unmagnetic material of the equation $\rho =$ constant, to an analytical formulation, we arrive at complicated mathematical expressions, the discussion of which, however, I must postpone for a later occasion. In general, in this case, the correspondence between ρ and F is no longer linear. That means:

The linear law of reluctivity, $\rho = a + \sigma F$, does not hold for all heterogeneous materials, except in a limited range."

A particular case of the reluctivity curve of a heterogeneous material, containing 70 per cent. of iron, is represented in Fig. 31. As seen, this curve differs greatly from a straight line, though being rectilinear at the initial part and becoming rectilinear again for very high M. M. F.'s.

But while the initial part of the curve points toward a saturation value, $L_\infty = 7.5$, being represented by

$$\rho_0 = .7 + .133\ H,$$

the higher parts of the curve point to an absolute saturation $L_\infty = 12.5$ and can be represented by

$$\rho_0 = 16 + .080\ H,$$

giving very different values of a and σ.

The most interesting question which arises here is *whether, and how far gray cast-iron behaves as heterogeneous material.*

For field intensities of 100 to 200 it does not do that yet, neither can it be expected, from the foregoing consideration. But with regard to very high magnetizations, the disagreement between Ewing's values of absolute saturation ($L_\infty \sim 16.0$) *observed* at extremely high M. M. F.'s and my own values, *calculated* from tests between 25 and 200 field intensity ($L_\infty \sim 11.0$), seems to point that way.

If a deviation of the cast-iron reluctivity from the linear law

exists, it must be expected at values of M. M. F. somewhere between $H = 400$ and $H = 2{,}000$ approximately. Unfortunately, within this range of M. M. F.'s, no tests seem to have been made with cast-iron. This point is open to further investigation.

THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, October 25th, 1892.

The 70th meeting was held this date at 12 West Thirty-first street, and was called to order at 8.15 P. M., by President Sprague.

THE SECRETARY:—At the meeting of the Council held this afternoon, the following associate members were elected.

Name.	Address.	Endorsed by
ARNOLD, BION J.	Consulting Engineer, General Electric Co., 4128 Prairie Ave., Chicago, Ill.	Elmer A. Sperry. Geo D. Shepardsan. Edw. L. Nichols,
JEFFERY, D. HERBERT	Secretary and Treasurer, Crocker-Wheeler Electric Co., 430 West 14th St., New York City.	F. B. Crocker. S. S. Wheeler. Gano S. Dunn.
MEYER, JULIUS	Consulting Engineer, North American Co., Mills Building, New York City.	F. J. Sprague. C. T. Hutchinson. Louis Duncan.
PEARSON, F. S.	Chief Engineer, West End Street Railway Co., 439 Albany Street, Boston, Mass.	Thos. D. Lockwood. Chas. A. Coffin. Eugene Griffin.
SPENCER, GEORGE JARVIS	Electrician, The Yale & Towne Mfg. Co., Stamford, Conn.	H. W. Leonard. Ralph W. Pope. Alex. S. Brown.
TAPLEY, WALTER C.	Electrician in Government Printing Office, Care of Public Printer, Washington, D. C.	Fred. W. Royce. Geo. C. Maynard. Ralph W. Pope.
VAN BUREN, GURDON C.	Electrician, 5 Wilson Street, Albany, N. Y.	F. L. Woodward. Wm. C. Miller. Wm. H. Hamilton.
WALLACE, GEO. S.	Telegraph Office Manager, Chesapeake & Ohio Ry. Co., Box 214, Clifton Forge, Va.	M. B. Leonard. J. W. Lattig. Ralph W. Pope.
WEAVER, NORMAN R.	Supt. of Repair Department. General Electric Co., 173-5 Adams St., Chicago, Ill.	A. L. Rohrer. J. B. Cahoon. E. C. Boynton, Jr.

Total 9.

The following associate members were transferred to full membership, their applications having been approved by the Board of Examiners.

PIKE, CLAYTON W.	Jas. W. Queen & Co,, Philadelphia, Pa.
BISHOP, JAS. D.	Electrical Expert, J. A. Roeblings' Sons Co., 345 W. 34th St., New York City.
BARBERIE, E. T.,	Electrician, Safety Insulated Wire Co., 234 W. 29th St., New York City.
HEINRICH, RICHARD O.	Electrical Engineer, Weston Electrical Instrument Co., Newark, N. J.
RAE, FRANK B.	Electrical Engineer, 27 Cleland Building, Detroit, Mich.

Total 5.

The next meeting of the Institute will be held on the 15th of November, when Dr. J. B. Williams, who read a paper on Insulation, at Chicago, will give some experiments in the testing of insulation in connection with his paper, which has not yet been published, but which will be in print before the meeting. It will be in the nature of a confirmation of the paper by showing the experiments, and also a discussion, which was not had at Chicago by reason of lack of time. In connection with these experiments, Dr. Williams will request that members bring or send samples of wire of their own selection, to be tested. The particulars in regard to these samples will be given out in advance, so that members may avail themselves of this opportunity. His idea in doing this is that some of the tests may be a revelation to either the manufacturers of wire, or other people, and therefore he would prefer that the samples be handed in by others than himself.

ELECTRIC RAILWAYS.

JOINT DISCUSSION OF THE PAPERS READ AT THE GENERAL MEETING OF THE INSTITUTE, CHICAGO, JUNE 8TH, ON "SERIES ELECTRIC TRACTION;" "A NEW SYSTEM OF ELECTRIC PROPULSION," AND "ELECTRIC RAILWAY MOTOR TESTS."

New York, Oct. 25th, 1892.

The 70th meeting was held this date at 12 West Thirty-first Street, and was called to order at 8.15 p. m. by President Sprague.

THE PRESIDENT:—There are three papers under discussion to-night—papers which were read at the Chicago meeting. The first is on "Series Electric Traction," by Mr. Nelson W. Perry; the second on "A New System of Electric Propulsion," by Mr. H. Ward Leonard; the third on "Electric Railway Motor Tests," by Professor Geo. D. Shepardson and Edward P. Burch. The discussion will be opened by Dr. Cary T. Hutchinson. There was practically no discussion at Chicago on account of the limitation of time, and the discussion to-night should be as full as necessary to bring out either the merits or the faults of the systems.

DR. CARY T. HUTCHINSON:—I had not the good fortune to be at the Chicago meeting, and therefore do not know exactly how Mr. Perry's paper was received; but I must confess that when I found such a paper had been read I was very much surprised; I thought this plan had been abandoned by everybody long ago. I dare say there were not very many people there who were entirely familiar with the beginnings of the series system and with the curious claims that were put forward for it at the time. A little history does no harm now and then. I have before me a circular issued by a series electric company in 1888 which is really unique and which I think should be preserved. It is entitled: "Why Is Electricity Not Used for Street Car Propulsion?" [Laughter.] The circular purports to give an account of the operating expenses of the three different

systems of using electricity—the storage battery system, the divided current, parallel, or multiple arc system, as it was variously called, and the series system. There is very little use in going into the storage battery question. We have not sufficient data yet to tell us whether he is right in regard to that. But when the parallel system is discussed some rather curious statements are made. I wish to read one or two. It is said, among other things, that "the parallel system works under the following disadvantages:

"First. The well known law that the power lost on a conductor is proportional to the square of the current on that conductor; so if it cost one dollar per day to send current enough over a given conductor to run one car, to run two cars twice the current must go over this conductor, which will cost not two, but four dollars; for three cars it would cost nine dollars; five, twenty-five dollars, and for ten cars, it would cost one hundred dollars to force sufficient current over these wires to drive them."

If that had been true we should not have run any cars. There are several delightful assumptions in this. The first is that the size of the conductor is kept the same, irrespective of the number of cars. The second is, that all the cars are bunched at the far end of the line, and the third is that the $C^2 R$ loss in the copper costs one dollar per day per car; as a matter of fact it costs a good deal less than one cent.

Then a second disadvantage is that "A system of this kind is subject to a complete stoppage of traffic at any moment, by a wire falling across the two parallel conductors, and every car on the entire system would immediately come to a full stop and there remain until the wire is removed. In the meantime, the generator at the driving station would have its armature burned or break its driving belt."

"Third. Upon this parallel system if a car was, for any reason, completely stalled, which is a common occurrence with street cars, either from an overload, being derailed or having the brakes set, and the current be then turned full into the motor, it would be immediately destroyed by having its coils burned out.

"In this system the motors have a large amount of power at or near the driving station, but the power falls very rapidly as the distance from the station increases, and upon comparatively long lines, the cars are scarcely able to move at the extreme ends of the line." [Laughter.] "Cars should have the same power throughout the entire length of the circuit, in order to allow them to run faster towards the outskirts of the city and make up their time.

"Each motor on a parallel circuit depends for its power and speed upon the movements of the rest of the motors on the circuit, making it vary in speed, and require constant attention from the car driver."

Touching another point, of which I may have something to say later in the evening, the circular says:

"The regulation of the motors operated on these parallel "circuits is accomplished by means of resistance coils being "thrown into and out of the circuit. As it always requires "power to send a current through a resistance, these coils are, "from their very nature, intended to be power dissipators, and "therefore consume coal without returning any useful equiva- "lent. They are the most unscientific contrivances ever devised "by man, and cannot be permitted in any perfect system of elec- "trical distribution of power. In addition, these resistance coils "are dangerous, as they become very hot, and have often set fire "to the woodwork of the cars." [Laughter.]

It adds:—"This system of distribution of electricity has been "tried for many years in incandescent electric lighting, and has "proved a financial failure [laughter] wherever many lights have "been run upon circuits of even moderate length."

The gist of the paper is the calculation of the cost per day of running twenty cars on a ten mile line. The only item, so far as I can make out, that is considered in calculating the cost of running the cars is the loss on the conductors. Of course this cost, at $1.00 a day, counts up. It shows a waste of 1455 horse power in the conductors and 250 horse power to be used in the cars; this requires altogether 2450 I. H. P. at the power-house to run twenty cars. The cost of this is something like twenty-five dollars a day for coal alone.

The series system has none of these disadvantages. It works at something like two dollars a day. Of course this all seems very ridiculous now, and at least one person knew it to be ridiculous then. Mr. Sprague at once wrote an article criticizing it, in which he said substantially just what I have said. He stated therein that the thing was actually done, and that it cost only about one dollar a day instead of twelve, and required 200 odd horse power instead of 2500.

Referring to Mr. Perry's paper itself, if anybody has followed all those diagrams through carefully as I have done, he must have seen that Mr. Perry has made an extremely ingenious arrangement of switches. As Mr. Perry himself says, the thing works beautifully—on paper. I might add that it never will work anywhere but on paper. Some kind of movable switches must be used, with magnets to open and close them, with two circuits on the magnets. Trolley wheels have to run on and connect three wires at the same time in many cases, making contact with all of them. These wires of course have to be insulated when the trolley wheel is not on them. Altogether the thing is extremely complex, and, I can say, absolutely impracticable. In addition to this, of course, there are the objections which apply to any and all series systems—very serious objections. In using a constant current one must vary the potential difference at the car; a car

which would take 100 amperes at 500 volts, as many do, will, if run on 40 amperes, require 1250 volts. As one cannot run a dynamo over 5000 volts with any sort of decency, it is necessary to section the line. The sections must be so short that it is impossible for two cars to be on the same section at the same time. For city work, therefore, the sections must be very short, which means more of these switches.

Mr. Perry neglects completely what probably is the most important objection to the series system—that is the motor itself. The constant current motor is a very different sort of thing from a constant potential motor. A constant potential motor can be run, of course, at double its nominal capacity for a short time, but when a constant current motor is at its point of maximum work—that is to say, when the brushes are in the maximum position—if the load is greater than that for which the machine is designed, it will at once come to a dead standstill. There is no margin of overload in a constant current motor, hence the machine must be built for the very greatest load it can ever possibly get. To regulate it one must vary the field or shift the brushes. Shifting the brushes causes sparking. One might shunt the magnetic circuit; but that means beastly magnetic design. Moreover, a series system is necessarily a double trolley system, and this of itself is enough to condemn it utterly, without regard to the numerous other objections cited.

Although it seems as if it were hardly worth while to resurrect this, yet certainly Mr. Perry does deserve a great deal of credit for the ingenuity with which he has worked it out. He has made a very readable article. It is well, however, to keep in mind that the only point at which it is even claimed the series system has the advantage is in the saving of copper. But the cost of copper, although a large item, is by no means the total cost, and even the reduction of the copper cost to one-half or one-fifth of its present value would not be such a great advantage as to counterbalance the other disadvantages which it entails.

The next paper is Mr. Leonard's "A New System of Electric Propulsion." I think, however, I shall leave this until I have finished with the other paper, because some things I wish to say about the other paper will have a bearing on Mr. Leonard's.

The third paper is on "Electric Railway Motor Tests," by Professor Shepardson and Mr. Burch. These experiments were made apparently in the shops of one of the railway companies at Minneapolis. They are evidently made with crude apparatus, and as far as I can make out from reading the paper rather carefully, the conditions of the tests are not accurately specified. In many cases one cannot tell at what voltage the test was made. No attempt is made to note the temperature; that is to say, the resistance of the circuits at the time the efficiency was measured; altogether I should not regard them as of the very highest degree of accuracy. Nevertheless I think the results given are

in substantial agreement with others. On page 579, in Fig. 1, is shown the curve of efficiency for the old No. 6 Sprague motor. The numbering of the clips is slightly different from what is usual; clip 2 would ordinarily be called clip 1. The tests were made on all the clips, that is 2, 3, 4, 5 and 7. As I stated some time ago, in an article I wrote, it is more than harmful, it is criminal carelessness to use these switches on any but the first, fourth and seventh clips, the reason being that the sixth clip has the same number of ampere turns as the seventh and a very much higher resistance. The same is true of the third and fourth. One gets practically the same speed with the fifth, sixth and seventh, but a very much better efficiency on seven. Many roads run on the sixth constantly. They are afraid to use the seventh, so they stick to the fifth and sixth and run with a resistance of about 0.6 ohms more than there is any necessity for.

I have compared these tests made by Shepardson and Burch with some which I published myself the other day;[1] they were published under my name, although they were really made by my company These tests were all made by an electric method. We adopted this method after trying in vain to get consistent results from Prony brake measurements; we tried various forms of the Prony brake, but were unable to get satisfactory results; impossible efficiencies were frequent. Finally we gave up all attempts to make mechanical measurements, and used the electrical method altogether. This method demands very few simultaneous readings, only two, and allows the correction for temperature to be made very easily. That is, one can reduce all efficiencies to the standard temperature, so that the efficiency curve is given for the standard temperature. Tests, to be strictly comparable, should be made under some such conditions as this. The suggestion, by the way, to make these tests at a constant resistance, and the method of deducing the formula, which is not as simple as it seems, are both due to Dr. Duncan. It will be seen from these curves that the No. 6 Sprague motor has an efficiency of 83 per cent. at 20 amperes, falling to 80 per cent. at 35, and running down in the other direction to 80 per cent. at about 12 amperes. That is to say, between 10 and 35 amperes the efficiency at the armature shaft is above 80 per cent. The efficiency with gears from 35 to 15 amperes is pretty constant at about 75 per cent. This is a very much better efficiency and better results in every way than I have seen published for any motor except some which apparently were distributed at the meeting of the street railway convention in Cleveland the other day. These were of the Short company's new gearless motor. Results are there given for four gearless motors, and one, if the curve is correct, is by all means the most admirable motor I have ever seen in the way of efficiency. It reaches a maximum of about 87 per

1. *Electrical Engineer*, Oct. 12, '92.

cent. at 7.5 H. P., and is practically constant to 14 H. P. The curve crosses 80 per cent. at 5 H. P. and again at 21 H. P., so the efficiency from 5 to 21 H. P. would average about 83 per cent. This motor then has a very high efficiency at low loads, a feature greatly to be desired. We all know that the street car motors run for nine-tenths of the time at very low loads. A motor which has high efficiency at low loads and drops down even to 60 per cent. at full load is very much better than one that goes up to 80 at full load and drops down to 50 per cent. at a quarter load. The authors of the paper call attention to the fact that these results are not strictly comparable among themselves for the reason that the voltage varied in the different tests. They give tests which I think none of us can interpret—such as Fig. 2—for the reason that the different curves there are taken with either two-thirds or one-third, as the case may be, of a T.-H. rheostat in series with the motor, the resistance of the rheostat is not stated anywhere, nor is there any means of getting P. D. at the armature.

Fig. 3 gives two rather interesting test curves of a No. 6 Sprague motor, showing how the efficiency at low loads can be raised by increasing the number of turns on the field; that is to say by working at a greater excitation at light loads than is usual. This is rather interesting and would be a very good thing if it were possible to work in that way. But unfortunately it does not take speed into account, really the ruling consideration. It is often said that the commutated field method of working is absolutely wrong, because it gives the maximum number of turns at the very worst time. The authors have brought out that point, and to a certain extent it is true. They make one observation on page 585 which I think is extremely pertinent and which everybody will have something to say about sooner or later; that is that an average of twelve tests on a road, showed that with two motors on a car it required 27 per cent. less current when only one was working. I dare say that most of us think that one motor is the proper thing now that motors can be so built that there is no longer danger of serious accidents. It is easy to see how the saving comes about. If from the current and torque curve of any motor one finds the current for a car under any conditions on the assumption of one motor, and then on the assumption of two, the result will show greater current when using two motors. This means simply that the motors are working at lighter loads, and consequently at lower efficiency.

I do not think there is much to be surprised at in the load diagrams given in Figs. 8, 9, 10 and 11. Although it is said that when there is a large number of cars on a line the loads tend to equalize, nevertheless on every road where I have watched the ammeter there has been an extremely large variation. I do not think that with any method of direct working, a variation of 25 to 50 per cent. can be avoided.

There is another point which Professor Shepardson touches rather lightly on page 585. He speaks of the comparative value of the rheostat and "commutated field" methods of regulating. There have been two other papers published recently, one by Mr. Thorburn Reid. the other by Mr. Wellman, touching on that subject. Mr. Reid says the motor with the commutated field is much more efficient; Mr. Wellman says it is not. I rather wished to be convinced that Mr. Reid's argument was right; but I confess I could not understand it, and I could not make much more of Mr. Wellman's.

This matter has been argued back and forth for more than three years, but in this time no one, apparently, has thought of appealing to experiment to settle it. It seems to me that the matter can be definitely decided very simply, and to this end, that we may hear no more of the subject, I have prepared the following table showing the case of

COMMUTATED FIELDS VS. RHEOSTATS.

C.	Position.	S.	φ	φ_2	φ_3	$\varphi_1 - \varphi_3$
20	VII.	9.1	84%	..%	84%	0%
20	IV.	6.3	69	63	60	9
20	I.	4.4	50	44	42	8
15	VII.	10.6	83	..	83	0
15	IV.	7.3	73	64	61	12
15	I.	5.4	59	48	45	14
10	VII.	13.3	78	..	78	0
10	IV.	8.8	75	63	60	15
10	I.	7.0	66	50	48	18

Here φ_1 is the efficiency of a No. 6 Sprague motor for the various current values and corresponding speeds taken from my tests referred to above; values are given for the three switch positions: φ_2 is the electrical efficiency with rheostat for the same current and speed. This is calculated by reducing the E. M. F. of the motor in proportion to the reduction of speed. The efficiency φ_3 is then the ratio of the reduced E. M. F. to 500. This of course takes no account of hysteresis, eddy current and friction losses. Putting these all at five per cent., *i. e.*, taking 95 per cent. as the efficiency of conversion from electrical into mechanical energy, we get the column for φ_3; that is, $\varphi_3 = .95\ \varphi_2$.

It will be seen that in every case the commutated field method gives an efficiency higher even than the electrical efficiency with rheostat, and much higher than the best probable value of the actual efficiency.

This comparison is made with the same motor and involves no element of doubt. There can be no question that with these motors the commutated field is the more efficient, and it is very clear to me that it must always be so.

There is one other point I will speak of. Professor Short pub-

lished on the 16th of April, 1892, in the *Electrical World*. an account of the practical operation of a gearless motor. This was either his first or second gearless motor. He shows as a contrast the efficiency of the double reduction motor on the armature shaft, on the intermediate, and on the car axle. I am sorry those efficiency curves cannot be shown, but the net result is that the efficiency of his gearing is extremely low. He calls attention to it by saying, "You will find on going over these curves that the load varies considerably with the speed." "You will find the efficiency is very low at heavy loads at slow speeds, and rises to its highest at medium speeds and medium loads." Of course in that particular case his object was, not to make a very good showing for the double reduction motor. Now taking that curve as he gives it, I find for a load of five horse power on the Short double reduction motor, on Professor Short's own testimony, the efficiency of the gearing is 78 per cent. only. We found on the Sprague motor 87 per cent. At 10 horse power his curve gives 74 per cent. as against our 92, and at 16 horse power his gearing efficiency falls to 63 per cent. as against our 91. He cannot be right; it is absolutely out of the question that it should be so.

The third paper is Mr. Leonard's: "A New System of Electric Propulsion." We all have heard this method talked of a good deal in its various aspects; I think Mr. Leonard has made a mistake in calling it a "new system of electric propulsion." There is nothing new about the electric propulsion. It might be called a "new system of electric regulation." The whole object is to replace the regulating apparatus of the ordinary street car equipment. A slight explanation of it may not be out of place, though I dare say everybody has read it. In Mr. Leonard's method there are three or four machines on a car. The first machine is a shunt motor run with a constant field, excited directly from the line. This I shall call the "line motor." The line motor is directly connected to another shunt machine which runs as a generator; the generator in turn supplies current to a third shunt machine (or to two machines) which drives the car axle, either through gearing or without it. The field of the generator is excited directly from the line, but has a rheostat in its circuit for regulation. The field of the driving motor is constant, and is excited directly from the line. The armature circuit of the driving motor thus has no connection to the line ordinarily. The regulation is effected by varying the field of the generator and thus varying the P. D. at the terminals of the driving motor, and hence its speed. The generator is driven at practically constant speed by the line motor, and hence its E. M. F. depends only on its field strength; this is varied within any limits by a small rheostat; it can also be easily reversed. The car will start very easily, and the current will not rise above the value required for moving the car. It gives a method of starting a car gradually, of running at any speed desired on any grade, seemingly without

the great waste caused by using up somewhere or other the difference between 500 volts and that which is applied to the terminals of the driving motor. To this extent it is extremely useful.

Mr. Leonard says: "If we could operate, from the constant potential system, a shunt wound motor running at a constant speed, and could interpose between this motor and the axle some device equivalent in its effect to an infinite number of different sets of mechanical gears, so that we could make use of any reduction desired, it would enable us, while using a constant power, to increase the torque as we decrease the speed, and *vice versa*, which is just what is desired in railroad practice."

As he says, numerous devices have been invented for accomplishing this variable mechanical reduction. I dare say there are half-a-dozen under way at present. Mr. Leonard makes out a very good comparison between the ordinary method and the method which he proposes, ideally considered. As we all know, the power required to drive a car is made up of two factors, the torque and the speed; consequently, even with a great torque the power may be quite small if the speed is kept low. If 60 amperes is sufficient to start the car, and the resistances are such that 40 volts will give 60 amperes, then only 2400 watts is used at the instant of starting, instead of $60 \times 500 = 30{,}000$. These figures, 2400 *vs.* 30,000 represent the ideal case; now let us see how nearly they would be approached in practice. Take, for purposes of comparison, the second column in Table 1, p, 571; *i. e.* eight tons at three miles per hour on a five per cent. grade. Eight tons at three M. P. H. on a five per cent. grade with one per cent. for traction is 7.7 H. P.; this is at the car axle. This is the output of the system—7.7 H. P.=5750 watts. Mr. Leonard specifies that his driving motor shall develop full torque with 60 amperes, and that its full speed shall be 12 M. P. H.; the armature resistance is .55 ohms. The counter E. M. F. at full speed is then 467 volts, and at one-fourth speed is 117 V. $117 + 33 = 150$ V. which is the P. D. at the armature terminals; $150 \times 60 = 9{,}000$ watts delivered to the driving motor:

$$\frac{5750}{9000} = 64 \text{ per cent.},$$

the efficiency of the driving motor according to Mr. Leonard's own figures.

The resistance of the generator is .55 ohms; therefore, its E. M. F. at 60 amperes is 183 volts and its output 9000 watts. Assuming an efficiency of conversion of 90 per cent., the input will be 12,200 watts. The efficiency of the generator is then

$$\frac{9000}{12{,}200} = 74 \text{ per cent.}$$

Taking the efficiency of the line motor as 80 per cent., a high figure, its input will be 15,250 watts; the efficiency of the system after Mr. Leonard's figures, interpreted, will be

$$\frac{5750}{15{,}250} = 37.6 \text{ per cent.}$$

I have in the above purposely ignored field losses. They are 560 watts, equivalent to about 4.5 per cent., and the net efficiency becomes 33 per cent. (say). This is as against 54 per cent., which Mr. Leonard arrives at by guessing the individual losses.

It will be instructive to show the efficiency of this same driving motor governed by rheostat.

Its output is 5750 watts: current, 60; counter E. M. F. 117 V., therefore, efficiency with rheostat

$$= \frac{5750}{60 \times 500 + 250} = 19 \text{ per cent.}$$

With commutated fields, from what I have shown above, it would probably be 27 per cent.

The difference between 33 and 27, *i. e.*, six per cent., is the gain in this case. This is trifling, and by no means worth the complication involved. It is clear on the face of it that in the first case, *i. e.*, running at full speed, the efficiency must be lower than the direct system. Mr. Leonard meets this by throwing the driving motor across the line when full speed is desired.

So far, I have used Mr. Leonard's figures as a basis of comparison, but in dealing with his third case—eight tons at one and a quarter miles on level—I am forced to depart from them. The figures given for the various losses are 10 watts for friction and 10 for other armature losses. Using these figures, I get

$$\frac{400}{480} = 83 \text{ per cent.}$$

for the efficiency or conversion of a 30 K. W. motor operating at one-quarter part of full load!

It must be remembered when considering these 10 watts for friction, hysteresis, etc., that this plan of Mr. Leonard's does not permit of gearless motors any more readily than the ordinary equipment does. Exactly the same considerations would determine whether a gearless or a geared motor should be used. Mr. Leonard states that he would use a gearless motor—why, I can't see. The efficiency in this third case would probably not exceed 10 per cent., instead of 29 per cent. as given.

There is one point of difference between this electrical arrangement of Mr. Leonard's and an actual variable speed gear—a point which it is worth dwelling on. A variable speed gear that will give an infinite series of changes between the speed of the driving motor and the car axle would be put, of course, between the motor and the car axle, and if one were content to run up heavy grades at very slow speed, a maximum of 15 H. P. might be used. The driving motor would then be only 15 H. P. With this arrangement of Mr. Leonard's the driving motor

must be exactly of the same capacity as it would have been otherwise; for this reason, on going up a heavy grade the motor takes its ordinary current—for instance, on a five per cent. grade 60 amperes. At some time it has to stand 60 amperes. It may have only 50 or 100 volts on it. At other times it runs at small current and at its full 500 volts. So taking these two considerations together, the machine must be of the same capacity as it would be otherwise. Exactly the same reasoning applies to the generator; that is to say, these two machines have to be of the full capacity of the ordinary car equipment; but with the line motor the case is different. Here there is the advantage of the saving in the actual power required to drive the car. In the line motor there is no need of greater capacity than the maximum power required on the car. If the limit is eight tons at three miles an hour on a 5 per cent. grade then the line motor should have a capacity of about 11 K. W., taking Mr. Leonard's figures in the second column of Table 1, or 15 K. W., taking my figures. Mr. Leonard, however, has ignored his own calculations, and made this a 7.5 K. W. motor; it will of course be much overloaded even assuming the correctness of his figures. I think that it will be found in all cases that the line motor should be the same size as the others.

One of the chief claims for this system is the reduction in power-house equipment. Mr. Leonard thinks he can use 6 H. P. per car at the power-house, and consequently greatly reduce the copper needed. This is based on the assumption that railway managers will be satisfied with such speeds as three miles per hour on heavy grades. This I believe to be absurd. There is the strongest demand for higher speeds, and 60 H. P. equipments are now put in, not to be safe in starting, but because 40 H. P. is actually needed to haul the heavy cars up the grades at the speed they must go to maintain a decent schedule. I dare say we have all heard railway men, *i. e.* non-electrical ones, praise a car because it went up a heavy grade at the same speed, approximately, as on the level. But even taking Mr. Leonard's 6.3 K. W. for a car on a level, and using 90 per cent. for line, 90 for generator and 87 for engines, it would require 12 H. P. instead of 6. This takes the work on a level as the average, and is under the mark.

I believe thoroughly in the desirability of a variable speed gear, provided it can be obtained without sacrificing too much; but I do not think this particular one fulfills that condition.

On page 576 is given a table intended to show the first cost of the two systems; from what I have already said, the following modifications of the table follow:

TABLE 2 (AMENDED.)

showing probable comparative first cost per car by present and proposed system; the latter as estimated by H. W. L. and C. T. H.

	Details of Plant.	Present System.	Proposed System.	
			By H.W.L.	By C.T. H.
1	Steam plant, generators and conductors	$2200	$1100	$2200
2	Motors, two 15 H. P. equipment	1800	1400	1800
3	Power converter	0	900	2400
5	Switches, etc.	200	30	30
	Total	$4200	$3430	$7430

I estimate the power converter as 60 H. P. at $40, not a high figure for tramway motors, and put the power-house output the same in both cases. This makes a total of $7,430 as against $4 200.

Another point which has not been brought out, and which works against the idea that the cars will use the same current, is the very much greater weight of this equipment. There will be say 30 horse power on your driving motors, 30 horse power for the generator and 30 horse power for the line motor, making 60 horse power additional machinery. This 60 horse power will weigh in the neighborhood of 6000 pounds. That is a load of 45 passengers, a heavy load.

On page 577 Mr. Leonard gives his summary of the advantage of this system. It reads:

"The features of the proposed system, which seem at first sight to be very objectionable, are:—The increased cost of the car equipment, and the fact that we are adding an additional machine, having two fields, two armatures and three bearings; but, as we have seen, there is only an apparent increase in the first cost, for the saving in the generators and distributing plant far exceeds the additional cost of the car equipment; and the use of the motor generators for elevators, for traveling cranes, etc., has demonstrated that, as regards the attention it requires and the depreciation it suffers, it has a marked advantage over the rheostat or commutated field used in the present methods of operation."

Against this I think the disadvantages are:

Increased cost of total equipment per car.

The adding of two more machines, with the consequent increased cost of repairs, depreciation and maintenance.

The greatly increased weight of the car, with the consequent increased power required.

The very small saving over commutated field regulation, due to the low efficiency of large machines lightly loaded;

And, greatest of all, sacrifice of simplicity.

The advantages are:
Excellent speed regulation.
Ability to return energy to the line.
And ability to use electric braking.

I should agree very strongly then with Mr. Leonard in the third paragraph of his paper in which he insists on the desirability of some method of accomplishing this object, but I should not approve the means adopted. [Applause.]

DR. CHAS. E. EMERY:—I discussed one of these papers in Chicago and did not expect to say anything this evening, but Dr. Hutchinson's remarks bring to mind some points upon which I have information that may be of service. It is well in all brake experiments to criticize the apparatus very closely. I have seen it recommended to throw a rope over a pulley, fasten one end and apply a spring balance to the other. Evidently this cannot be accurate, except when the fast end of the rope is slack, and yet sufficient friction remains to show a tension on the spring on the other side. A rope passed with several turns about a pulley, and provided with a spring scale at each end, has been very strongly recommended, but the two scales must be read exactly at the same instant, if not absolutely steady. It would evidently be better, therefore, to replace one with a weight. The Appold brake, which has been spoken of in connection with the experiments of Prof. Short, was much used in experiments with agricultural engines twenty odd years ago. The tension is self-regulating; the two ends of the strap connect to the short end and fulcrum of a lever, and the long end is held stationary. One side of the strap is loaded, and tends to move the lever in such direction as to tighten the grip. When the turning effort of the wheel is sufficient, the weight is lifted, and the short end of the lever carried around until the strap is loosened so that the wheel turns within it, and the adjustment is thereafter automatic. This arrangement has been criticized from the fact that a portion of the force required to strain the belt, is received by the stationary point at the end of the lever, and as its amount is not known, it cannot be deducted from the load as it should be. In an article on dynamometers written for Appleton's *Cyclopedia of Applied Mechanics*, I showed that the above defect could be obviated either by putting a spring scale at the end of the lever, or by the use of an additional lever which automatically subtracted the force required to produce the strain. The article is referred to for further information. I have seen a great many clamp brakes, merely screwed up by hand, which operated quite reliably. I have found it of advantage, however, to have a spring under one of the bolts, or otherwise provide means to make the grip on the pulley elastic.

I happen to have some information on another point that may prove interesting in this connection. The desirability of having a variable gear, which Mr. Leonard speaks of in his paper, and

which has been referred to by Dr. Hutchinson, has been attempted by a great many, among others by myself, and my experience may be of value to others who are thinking in the same direction. If we use an electric motor to operate a pump of variable capacity, and then with the fluid operate a hydraulic motor on a car axle, the variation in the quantity of fluid delivered in a given time will evidently vary the speed of the car axle, and we have the variable gearing, or at least the variable mechanical connection desired, subject to the disadvantage of friction in the apparatus. It was thought that by properly proportioning the pipes so as to utilize the high fluid pressures now in vogue, probably the saving electrically would be greater than the mechanical loss. I spent no money in testing this fact. I first determined to make a reconnoissance and ascertain what had been done in the same direction. The simplest way of doing this, as I had found in other cases, was to put an application in the Patent Office complete enough to cover the general idea, which brought out the fact that there had been dozens of applications on the subject; that the general principle had been well covered years ago, and that a number of interferences were pending with parties who proposed to use modifications of the system. I, moreover found in this way, a man who had actually made tests on the subject, who had actually used. on a car, a pump with variable capacity to drive a hydraulic motor fixed on the axle, but the fluid soon became so highly heated that it was necessary to stop every little while and allow it to cool off. To be sure, the inventor ran the pump with a steam-engine instead of an electric motor, but evidently this had no effect on the function of the fluid connection. The sizes he selected were, moreover, nearly the same as I had fixed upon in my mind as being about right, he using a maximum pressure of about 300 pounds, and distributing the fluid through four-inch pipes, while I had expected to use about 500 pounds and pipes of the same size. This difference may be sufficient to overcome the difficulty, or at least it may be practicable to use large pipes and high pressures with smaller volumes and prevent serious heating. The car as equipped, is housed within a few miles of New York, and I could readily obtain permission to apply an electric motor to it, but on account of the difficulties above mentioned, and the fact that the fluid gearing was completely anticipated, I have taken no further interest in the matter. Curiously, I have met Dr. Edison within a few days, and in the course of a conversation he outlined an almost identical arrangement which he said he had never had time to work out, but which he assured me was the right way to do it. There were a number of persons in interference with the inventor I have referred to, who proposed to employ the same idea for clutches, permitting the fluid from a pump operated by the motor to escape freely through a shunt opening in starting, and gradually closing such opening to bring the speed of the motor and driven shaft together. I have heard from other sources that

some of these devices heated badly, but in such case this would not be so much of a disadvantage, as the device is practically only used in starting, like a rheostat, and considerable time will be available for cooling.

THE PRESIDENT:—May I ask if the method of changing the capacity of the pump was by varying its internal arrangements or simply varying the speed of the pump?

DR. EMERY:—He could vary the speed of the pump but that was not in either Dr. Edison's mind or mine.

THE PRESIDENT:—Was there any attempt to throttle the passage of the liquid?

DR. EMERY:—I think he used a shunt. No, he used several pumps of different capacities, and by means of a change valve could readily operate either, or all, at will.

The only other point which it occurs to me to mention here is, that while we admire the regulating system of Mr. Leonard as presented in his paper, and we expect he will probably be able to defend it from the practical criticisms that have been made, still he has a very strong competitor in shunt motors, which, though thoroughly understood, have not to my knowledge been heretofore applied in such a thorough way as recently by Messrs. Siemens Bros. and Co. on the new locomotives of the City and South London Electric Railway.[1]

Apparently these motors have been made heavy enough—in other words, iron enough put into them—so that considerable changes of speed can be made by simply varying the strength of the field without any material sacrifice in economy, and the dead resistance of a rheostat is only used for a few seconds in starting. At first sight this would appear to be a system applicable to street car motors, but on careful consideration it will be seen that there must be a sufficient section of iron in the field and armature, to utilize the whole strength of the field at a reasonable point below saturation so as to obtain the low speeds satisfactorily, and there must be a sufficient number of ampere turns on the field at the high speed to prevent sparking. It does not appear that both these conditions can be obtained without the use of more iron than is ordinarily employed with series motors of less range. Such an arrangement should not, however, be objectionable when the motors are put upon a special car to form an electric locomotive.

MR. C. O. MAILLOUX:—I have but very little to say on this matter; because it is mainly a branch of the subject where I am, so to speak, tabooed, as I have had the misfortune (so my friends say) of being connected with one phase of the electric traction problem which is not popular just now—though I think it will be more so later on. However, there is one point which occurred to me as Dr. Hutchinson was speaking; it is in relation

1. See *Electrical World*, Aug. 27, 1892.

to facilities for regulation as affecting the probable success of the storage battery. If we stop to think for a moment we realize that the storage battery is certainly not deficient in regard to the means of regulation which it affords. In fact that is one of its strongest points. If it were as strong in all other points as it is in the facilities which it affords for regulation, there would be no trouble and no doubt as to its success. One of the readiest means it affords and one of the earliest ways which was thought of for regulating storage battery motors, was to divide the storage batteries into a number of groups and to group the circuits in parallel at the start, and then gradually group them into different arrangements of series, multiple and so on. In that way you get a variable electromotive force, so that you attain, to a considerable extent, the very object which Mr. Leonard undertakes to obtain with his machinery, viz.: a variable electromotive force of supply.

But there is another point coming in there which I think argues in favor of using two motors on a car, even though there may be, as we recognize, an economy in using only one. The use of two motors lends itself beautifully to regulation, as it enables us to start with them in series, and to couple them in parallel later, and play similar changes with the batteries, and thus we get a whole chain of regulation, so that we can get the same thing practically as if we had four or five different initial electromotive forces. In other words, if we have an electromotive force of 200 volts maximum with all batteries in series, we can start with what virtually amounts to about 20 volts electromotive force by making, say, four groups of batteries all connected in parallel at the start with the two motors in series; and we can successively couple the motors in parallel, and the batteries in series, for increasing the speed, and finally reach the maximum potential. The method of using batteries in parallel and motors at the same time was first tried, I think, by myself on Fourth Avenue in 1887, and has been used more or less since by all having to use storage batteries. It has not come into use in connection with trolley roads until quite recently. I think that it is only this year that it has been agitated and brought into practical use. However, to-day there are very few roads that do not practice it and with considerable success. I saw some later forms of regulators by the companies exhibiting at Cleveland which showed a marked increase in the extent to which combining commutation of the field and commutation of the motors themselves has been used as a substitute for resistance in getting the intermediate steps between starting and full speed.

I might say something in relation to the variable speed ratio method of regulation, because, as I was saying to a friend of mine at Cleveland, it is like the measles—I think we all have to have it; and I do not suppose there is any electrical engineer who has given any considerable thought to electric traction but that has

at some time or other in his career come across several attempts at variable speed ratios, and perhaps has devised three or four himself, and tried a few. I have not been more fortunate than others in having to undergo the experience. I have also tried some. The conclusion that I have come to is, that while there is a fancied or perhaps a real trifling economy in their use, as long as we are able to accomplish substantially the same object in other ways, namely by varying the number of cells in series by grouping the sets, or else by varying the motors, or both, it does not pay, while, moreover, the extra complication is very objectionable. The cost of coal is not such a large item after all, while the amount of money involved in the depreciation is usually a very important item which frequently exceeds that of the cost of coal. It is, therefore, a serious question whether it is worth while to think of using such methods, at least in the present state of the art of mechanics. It is possible that some new application of mechanical principles that we do not know of yet may be discovered, which will enable us to accomplish in a simple, uncomplicated way what we all dream of as an ideal, but I know of nothing of the kind now. The nearest I know of is a method which Dr. Edison published of an epicyclical gear, the two movable members of which are each operated by motors. I happened to work in the same direction for a long time. and probably should have had an interference with Dr. Edison on the subject if I had been a little more diligent in the direction of patents. But I am not at all sorry. I am willing Dr. Edison should have all the glory there is in it. I tried the same device on a street car, though moving only one of the gears of the epicyclical gear train and having a friction clutch on the other movable one. By that means you can obtain any ratio from infinity down to the ratio for which the gear train is arranged. I found that the device worked very satisfactorily, but the complication was objectionable, and the friction band on the loose gear and the difficulty of keeping it in adjustment under all conditions, and things of that kind were objectionable. The motorman very soon got out of the habit of using it at all if he possibly could avoid it. The motorman seems to find in practice that the fewer handles and levers you give him to manipulate the better off he is, even though I had this arranged so that it could be operated with the brake handles. The man would put on his motor and it would start it running, but with the clutch loose. Then by turning his brake in the reverse direction the clutch was gradually put on and the motor gradually clutched on, and the ratio gradually decreased from infinity down to its normal value. The car very nicely started and followed on. But I found that even this arrangement, though as simple as it could be, was not simple enough for the average motorman, because oftentimes he would start the wrong way to do it.

At any rate I consider that I am cured of the fever for some

time to come, and I am willing to be content with systems of regulation, perhaps less economical of power, though also less expensive to build and to maintain.

DR. EMERY:—I wish to ask one question before the subject changes. I understand that series parallel, or series multiple traction is pretty well covered by patents. If my information is correct I do not see how the various applications spoken of by the last speaker can be made by engineers generally and will be pleased to hear the statements of others more fully informed on the subject.

[Vice-President Hammer took the chair.]

MR. SPRAGUE:—If I can depart from the chair for one moment I will make one reference to this subject. The series system was tried at Richmond five years ago. Twenty cars were fitted with multiple series switches used with the commutated fields. They worked perfectly so far as increased torsional effort, the slowing down of the machine, and all that, was concerned. The method was not patented. Where it failed was when on heavy grades the attempt was made to run the machines in series, and to start on a slippery track. In that case, if there was slipping on one portion of the track on account of ice or sleet or for any other reason, one motor would begin to race and the other motor would slow down. The claims made for series multiple regulation have afforded me a good deal of amusement. As to the effectiveness there is no question, but the series multiple system, when running at full service, will make little difference in the coal burned. So we simply threw away our series multiple switches and ran our machines invariably in multiple. The action of two machines in series is easily understood. Of course, the total counter-electromotive force built up, is the sum of the counter-electromotive forces of the two motors, and if one runs fast, the other must necessarily run slow, so that sometimes we had one running one or two hundred revolutions a minute and the other standing dead still.

MR. MAILLOUX:—We are never troubled that way with storage battery cars. They are always heavy enough to give plenty of grip with the rails.

MR. SPRAGUE:—In the latter part of 1886 Mr. Wharton of Philadelphia was supplied by me with a couple of motors operated on the storage battery system, and the switches supplied provided not only for a change in batteries from operating four sets in various combinations but also for multiple and series arrangements of both armatures and fields. It was not very satisfactory, because it was found altogether too complicated.

PROFESSOR FRANCIS B. CROCKER:—One point that Dr. Emery referred to, I think can be explained, and that is the use of shunt motors on the City and South London Railway. On any line where you have a clear track there are usually only three conditions to consider. One is standing still, one is starting, and the

other running at full speed. Now those three are, any or all of them, easier to take care of than running at very slow speed for a long time. That is the hardest thing to do for an electric locomotive or electric street car. That is what strains the rheostat, reduces the efficiency, and puzzles the electrical or mechanical engineer most. But when there is a clear track, as in the underground railway, the tunnel of which is used exclusively for this particular railroad, they can run nearly always at full speed. I happened to ride on that road this summer. It takes perhaps half a minute to start, and then they are at full speed. Consequently there is no period of any length that is considerable, where they are running at slow speed. It will be seen that the losses, the inefficiency of commutated fields or rheostats, etc., are not to be considered at all, because they enter for only a few seconds, and this is a very small part of the run, and a still smaller part of the actual work of the system. So what is true of a train that can always run at full speed when it gets started, is by no means true of a street car system where perhaps you may have to run at half speed, or even quarter speed for a long period of time, if you are behind a truck or going through a crowded street, or under various conditions that will suggest themselves to any one who has ridden on a surface city railroad. The experience on the City and South London Railway would be a very poor thing to adopt as the standard of street car practice. On the other hand, the success of this railway is a very encouraging thing to the electrical engineer, who looks forward to seeing electric locomotives used on long distance service, where we have a free right of way and the ordinary condition of working is full speed. I think this is quite an interesting point, and one which should be borne in mind by any one who is putting in the street car system, or one who is putting in the long distance or other "clear track" system.

In regard to the relative efficiency of commutated fields and rheostats, I early conceived the idea that it is very objectionable to have this dead resistance. In fact in 1884 I was one of the patentees of several inventions which had for their principal object the elimination of the loss in the dead resistance, and commutated fields was our system. But this is somewhat a question of words. That is to say, the resistance exists there, and we have got to consume the difference between the counter-electromotive force and the direct, or most of it, in some way or other, and there is no very efficient way to consume it. We can use it more or less satisfactorily in exciting the field. But since a motor will work with such a small percentage of current in the field, it is pretty obvious that we have got to consume a considerable amount of energy in our dead resistance, if we use it, or in our system in some other way if we try to avoid dead resistance. Of course the rheostat, although it is the simplest form of using up this energy, is not the most efficient in theory, and perhaps not in

practice. If a certain amount of that lost energy can be saved, why so much the better; if not, we have got to bear with that loss until some better system is devised.

DR. EMERY:—In regard to the subject of shunt motors, I quite agree with what Professor Crocker says, but evidently he has not read the article to which I refer. The efficiency tests of the Siemens motors referred to were necessarily made in the shop with a brake resistance, and the different speeds given, merely translated into car speeds by calculation. These tests showed an efficiency of 90 to 92 per cent. for variations of speed between 12½ and 30.6 miles per hour. Complete diagrams were given, showing the particulars as to actual speeds, volts and amperes. The maximum speeds in practice varied generally from 17 to 19 miles per hour according to the direction of the grade, and the averages for the round trips, exclusive of stops, were about 13 miles per hour. I find it possible from the changes in the initial slopes of the velocity diagrams to determine, with reasonable probability, the several operations performed by the engine runners; for instance, it can be seen when the dead resistance was probably taken out and the motor therefore running with full field and the acceleration a maximum. Soon, however, the speed rises more slowly and other changes may be observed, showing doubtless when resistance was put in the shunt field so as to further increase the speed.

MR. CHARLES HEWITT:—The use of the shunt motor for street cars has not been entirely neglected in this country, and is certainly not original with Siemens and Halske. I am not sure but that the President has tried it in the early days, and although Siemens and Halske may be able to use their locomotive successfully in London on that particular road, it certainly will not be a success here, until we get a much smoother running trolley than we have at the present time. Any one noticing a trolley running here will find that at almost every hanger there is a slight sparking. That is due to the slight contact and sometimes no contact at all, due to the jolting of the trolley from the wire. I have been on cars with a voltmeter when I could count every clip by the swing of the needle. With a shunt motor every time the trolley leaves the wire, the shunt field is discharged, and when it strikes the wire again you get a short-circuit through the armature. This trouble is still worse when the trolley wheel jumps off, as it frequently does at switches. I believe in London they use a third rail and a top-running trolley wheel, perhaps two of them, and are sure to get a contact all the time.

With regard to Mr. Leonard's arrangement, I do not intend to go into any details of it except as to the complication involved. We all know the difficulty of having two motors on a car properly cared for. Only recently, within a very short distance from here, I knew of a case of a hot journal where the motorman got off, scooped up dust in his hands and threw it in, and said that

that would cool it off. When we put a complicated arrangement such as this must be, into the hands of men like that, we increase those difficulties very much. The whole tendency of street car practice to-day, it seems to me, is toward greater simplicity. I am very nearly convinced that we are coming to the day when we shall use a single motor on street cars—I won't say on locomotives, but on street cars—for small powers.

I want to call attention to what Dr. Hutchinson spoke of on page 585: "An average of twelve tests on the road shows that with two motors on a car it requires 27 per cent. less current if only one is working." Dr. Hutchinson seemed to think that that was chiefly due to the motor working at a very small part of its power. From a great many tests which I have made myself and have seen others make, the average power taken by the motor when it is actually doing work is somewhere in the neighborhood of 50 per cent. of its rated maximum. You take these tests that are shown here—I have not averaged them, but I have averaged a great many others. The No. 6 motor, I believe, is wound for 25 amperes. On a great many tests I have found that the average current while the motor was actually being worked varied from about 20 to 30 amperes per car, that is the average of all the readings. Say the average for the car is 25 amperes, and assuming that each motor does its proportionate share, that would be 12½ amperes per motor, or 50 per cent. of the full load of the motor, and at 50 per cent., granting that this curve here is approximately true, the output would be seven and a half H.P. They have carried out this curve farther than we usually consider it. Say the motor is a 15 H. P. motor, at half load it gives over 80 per cent. efficiency. Even if the power used is only between five and six, you see the efficiency is over 80 per cent. So that I do not think the difference between the one and two motors is due so much to the low efficiency, as it is to the difficulty of making two motors run together as the motors on this road were connected. It is a well known fact now, that with two motors, connected according to Mr. Sprague's design, with a connection from one motor to the other between the fields and the armatures, it is very difficult, especially with the double reduction type, to get them to equally divide the load. This difficulty brought out the equalizing coils which were invented, I believe, by Dr. Hutchinson. The matter has been simplified more recently on the new motors by giving each motor a distinct circuit. I have known cases where not only one motor has done no work at all, and the other has done all the work, but cases where one motor has turned into a generator, and generated as high as 10 or 11 amperes, and the other done all the work. So that the difference between one motor and two motors in that case would be very apparent, and not due to the low load at which the motors were working. I have had such a case as that within a couple of weeks.

DR. HUTCHINSON:—Of course I know that a great deal of the

loss is due to the fact that one motor drives the other as a generator. But still if you take the efficiency curve at the car axle you will find a difference of about 15 per cent. But the tests given by Professor Shepardson and just quoted are tests on the armature shaft, not on the car axle, and consequently not applicable.

MR. MAILLOUX:—I would like to ask if Dr. Hutchinson has any information on motors when they are equally balanced by having each run with an independent circuit. I was astonished myself to discover that this method of running independently was not sooner brought into use, because I have never run motors in any other way but in an independent circuit for each. Of course, it would be apparent right away that there would be some difficulty. I would like to know what difference there is in running them balanced, each motor having its own circuit.

DR. HUTCHINSON:—You have a certain total horizontal pull. You divide this between two motors and find the current for one motor and multiply it by 2 The difference would be about 15 per cent. It is hardly worth while going into, but you will see the point at once.

MR. SPRAGUE:—I have a few remarks to make on those subjects. Those who have visited the beautiful Moravian cemetery in Salem, North Carolina, or that in New Orleans, where they bury the dead above the ground in tombs, or in country towns where lovers stroll on Sundays, have doubtless often noticed one inscription on the stones which mark the resting places of those who in life perhaps were filled with great hopes and ambitions. That inscription is "*Requiescat in pace.*" When in Chicago some four months ago I heard Mr. Perry's paper on "Series Electric Traction," and Mr. Leonard's on "A New System of Electric Propulsion," that thought came to me—better let them "rest in peace." In these four months there has been no resurrection. I feel now as I felt then. If there was good in these systems it would work out its own salvation, and if the defects which seemed to be apparent were there, they would of themselves eventually condemn them. So satisfied am I on that I see no reason to change my opinion. The series system of electric propulsion is absolutely dead. Every now and then an attempt is made to revive it. It has been tried on one or two roads in England. It has been tried on one or two roads in this country, but if I may make reference to Prof. Short's own work, his experience has been a suggestive one. He started five or six years ago on the series system in Denver and elsewhere, and about four years ago in West Virginia. It is dead. And then he went on to the double-wire multiple arc system overhead, as did the Thomson-Houston company in Cincinnati to accommodate the local requirements of the telephone interests, and that plan is practically dead, and we have come, on the five hundred railroads in operation in the United States and elsewhere, to

one single method of operation, a multiple arc system of distribution with a single wire overhead—and that is where we will stand. I have no hesitation in putting myself down as a prophet in some of these matters. There have been great advances made in the last three or four years in electric railways. My own connection with them for the past two years has not been active. I am now going into a different class of work. But while there have been improvements in price, and improvements in mechanical applications, more perfect machinery, greater interchangeability of parts, there has been little if any increase in the actual economy. Four years ago forty cars were running on the Richmond road with engines of 375 horse power, in other words, nine horse power per car at the central station. Every car on that line was doing all that it could do. We have in fact retrograded in some respects, and the number of accidents which are to-day cited, sometimes justly and sometimes unjustly, in opposition to the trolley system, is due in a large part to turning backward, and to the additional complication of apparatus. The plan which seemed to me to be the proper one was to give the man who ran the car, as little as possible to think of. All that he should be required to do is to move a lever—as small as convenient—one way to go ahead and to pass through all the gradations of starting, increasing the speed, going at full speed, and by reversing that movement by a single stroke of the hand to pass through the retrogression of those changes, to reverse his machine and to run in the opposite direction. That plan has been abandoned. Two levers must now be used, and many a life has been the forfeit. The demands of street car service have continually increased. We can no more creep up a five per cent. grade at three miles an hour to-day than we can put a horse on the car to pull it up. The schedule time of roads will not permit of that operation. At first it was 15 horse power to a car; to-day it is 20, 30 and sometimes a 40 horse power equipment. Why? Not because of the requirements at starting, but simply because when running at the full speed at which the machine will run when loaded, with the cars, with the trucks, with the passengers carried, the motors are required to do that amount of work when working under the highest possible efficiency for which they are built. We cannot go back.

Referring a moment to the question of control, I have noticed in recent observations of some of the roads, that the motorman is required to operate two sets of apparatus. He has his regulating apparatus for varying the speed of the car. He has his independent switch apparatus for varying the direction of motion. What is the result? A man is pulling along at 12 or 15 miles an hour with a tow car behind him. He is travelling a car length in a second. Another car comes along in an opposite direction, and some man dull of hearing or short-sighted steps in the way. If the car is controlled by a single movement, which

responds instantly to the wish of the motorman, he has got the power, independent of his brakes and quicker than they can operate, of bringing his car to rest before he has run over that passenger. But if he has got first to cut off his current, move the reversing switch, and put on his current again, he has passed double the length of the car before he can stop it. I say in that respect we have retrograded in street car work when we have added two movements instead of running with one.

I had intended to comment in detail on this system of Mr. Leonard's. Independent of any possible merits of the plan, there are, I am sorry to say, many inaccuracies; some have been pointed out by Dr. Hutchinson, and there are others which Mr. Leonard himself may notice in looking over his notes. There is much assumed in the matter of economy, but when we consider the time limit involved in starting, I unhesitatingly assert that the amount of coal burned in the central station will be fully as great, if not greater, by this plan, than with the system of direct supply, because of the amount of energy needed for the continuous rotation of the power converter.

He has made one reference to elevator work. It is an unfortunate allusion. An elevator is in some respects the hardest sort of railroad service. I have a record made over a period of about eleven months on an elevator driven by a motor which is a compound machine, governed purely by a rheostat. This motor runs for ten hours a day. It is a very fair example of good elevator service so far as the element of time is concerned. It stops at five floors of a building, carries both freight and passengers, and is practically in continuous service. Its operation is much more continuous than very large numbers of hotel elevators and many office building elevators. That record shows that for ten hours duty a day there is used at the terminals of the motor the equivalent of about 100 amperes at 240 volts for one hour. These are from meter records, and meters were never known to lie in favor of the customer. We run about 250 feet a minute, stopping at every 10, 20 or 30 feet, and oftentimes running slowly. There is not used in the rheostat itself over ten per cent. of that energy; that is, 10 amperes at 240 volts for one hour. An elevator of that lifting capacity, if driven on a three motor scheme, would have to be supplied with a motor and dynamo of at least equal capacity driven by a motor of about 30 horse power. Two of these machines would normally, of course, have to run all the while. Of the size specified, they could not be run from a 240 volt circuit for less than about 15 amperes. Consequently we have the equivalent of 150 amperes at 240 volts for one hour. In other words, for the system of regulation alone to take the place of the rheostat, there would be used 150×240 watts against a total present use of only two-thirds of that, and against a use of one-fifteenth part of it for the rheostat. So I say this is probably a clear illustration of how the time element enters to

continually cut off the efficiency of the system where you have anything that runs continually.

With regard to the method of operating by shunt machines, I would say that in 1886 on the elevated railroad I operated two machines which were of this character. Subsequently they had a small series field introduced. The speed of the car was controlled there first by the rheostat, and second by variation in field strength, and part of the energy of the car was delivered back to the line in slowing. Later, if I remember rightly, we introduced the series method of regulating the machines as well. In Richmond, the following year, I did do that. With machines of even ordinary weight, that is 100 lbs. to the horse power, if the series multiple method is used with the armatures alone, and there is a variation of the field strength, you can get a variation of from one to three in the speed without any difficulty whatever, and that eliminates all difficulty in starting. In other words if the maximum speed you wish to run is 15 miles an hour, you can easily run at a high efficiency at five miles by simply strengthening the field and throwing the machines in series where you have got a good track and a sure method of contact. When taking into account the fact that you can return a certain amount of energy to the line, I question very seriously if the shunt method will not hold its own in time even on a great many lines of street car service.

The difficulty which Mr. Hewitt has pointed out, that the two motors were not in ordinary street car practice run on different circuits is explained in this way:—When motors were first built, there was an assumption that the motors could be built to run alike. The current was made to divide itself in the two fields equally and then meet and divide itself in the armatures. Afterwards we found the difficulty which is pointed out by Mr. Hewitt, but it was too late to change our switches, because we had some thousands in operation. In fact we were under the same conditions that every manufacturer of apparatus made under stress is. If the two circuits are separate, then a large part of the difficulty, I should say three-fourths if not more of it would disappear at once.

Dr. Hutchinson:—There is one word which might be added to what Mr. Sprague says about speed on grades and the power required in the power station. We are running a line, as he knows, in Baltimore, on a 6 per cent. grade and carrying cars up that grade at eight miles an hour. Then there are in use 60 horse power equipments on a car instead of 40. We make on that road a daily mileage of 205 miles with one car, the highest I ever heard of. The rest of the cars make 190 miles. I have in my hands a lot of tests which I made two years ago, showing the indicated horse power per car measured from engine cards on the old Sprague double reduction motors. The lowest was 6.2 I. H. P.; the highest, 8. If you measure the same thing now you will get anywhere from 12 up to 18 and 19 I. H. P.

MR. MAILLOUX:—There is one point which I think has been neglected entirely in connection with this discussion; though it has perhaps equal importance, at least, with the methods of motor regulation so far as the economy of running and cost of the power is concerned. I said a little while ago that the cost of power itself was very small, and it was usually smaller than the cost of the depreciation. It seems to me that if we want to make it still smaller, if we want to reduce the cost of power pure and simple still further, that there is a much wider margin to work upon in the station itself than in the cars or motors. A number of engineers have investigated this subject, and it has been shown that owing to the constant fluctuation of the load, and the fact that the power is constantly varying; that the engine is cutting off at full stroke and then at very short stroke, in very rapid succession, the average (mean effective) economy, so to speak, of the engine, is very poor. For that reason, more than any other, compound engines have not turned out the results which were expected of them, and we do not obtain from the coal used, the horse-power that we ought to—not by 15, 20 or 25, sometimes 40 per cent. It seems to me that it is there we must look for any radical improvement in the amount of power consumed to propel the cars. The amount of energy wasted in starting, as has been sufficiently shown, is relatively insignificant, but in the station we have a large margin to work upon—a leak at the bung hole, as it were, as compared with the spigot (regulator) loss. I believe various methods have been submitted for increasing the station efficiency. I advanced one myself some two years ago, which has not been popular because the storage battery was used as a factor in it. There is, in my opinion, a good time coming when the storage battery will have its innings, and will be used in every electric power generating station; but meanwhile there may be other ways available or possible of equalizing the load, and thereby increasing the efficiency of the plant. A method has been advanced, depending on a peculiar form of steam-engine, by a concern which makes an engine that is said to give almost constant economy under very wide ranges of load. The makers claim to have been able with their engine to reduce the water consumption for given duty from 40 pounds per horse-power per hour, to 26 and 30, which results, if correct, ought to receive the attention and consideration of electrical engineers.

MR. SPRAGUE:—I may speak of one or two matters that have recently come to my knowledge. As regards the amount of coal used in central stations, it is undeniable that the central station presents great possibilities of improvement. People who are building to-day any large isolated plant can easily get from engine builders a guarantee of a horse power delivered to the shaft of a dynamo directly connected for not exceeding 25 pounds of water, and the dynamos are now rated at 92 per cent. We are going to have increased efficiencies in our central stations when we adopt direct connected machines.

As to the method of regulating larger machines, my own impression is this:—That for the larger railroad work of course it is essential that there should be at least two, and possibly four machines in operation, on account of the weight, methods of attachment, etc. The series multiple system will be used for running at variable speeds. The machines will be compound wound. The rheostat will be a very small factor. I am now concerned with some others in building an electric locomotive of about 700 horse power that has four motors of 175 horse power each. The adoption of four machines in place of two was determined, not by the difficulty of building two machines, nor by the difficulties of regulation, but simply because the space at our command required that arrangement. Suppose these machines run at 30 miles an hour under full load and best conditions, that is when all four are multiple; they will run about 14 miles an hour when run two in series and two parallel; they will run six miles an hour when all four are in series. When you get down to six miles an hour you have got down nearly as far as you want to go. If now we have in addition, the method of control by varying the field, we increase these variations of speed at least 50 per cent. I think that in the work now being done by the General Electric Company for the B. and O. R. R. under the original Thomson-Houston plan they will use four or six machines in these various combinations. It seems to me that the best method when properly connected is to run them in series multiple combinations. There is no difficulty in changing from one to the other provided you have got proper field regulation in addition.

MR. H. WARD LEONARD:—Mr. Chairman and Gentlemen:—I think that never since I first thought of this system of regulation have I felt quite so much encouraged as this evening, for after having had my figures, for several months at any rate, and having examined them, that Dr. Hutchinson should find that I only claimed 97 per cent., and when he is willing to grant that it may possibly be 20 per cent. and upwards, it shows me that I have something to work for, and with the possibility of success. The fact, joking aside, in regard to the question of efficiencies, is that quite accurate tests have been made of the system by a concern so well equipped for making tests as Wm. Sellers and Co., of Philadelphia, and that the efficiencies shown by the tables in this paper have been quite well borne out. I may say, while I am on that subject, that the losses in the fields and the friction, and the Foucault current and hysteresis losses which I have assumed, are derived principally from the tests of the Franklin Institute, and are also checked by the data of the test averages of the Edison dynamos, and I think they will bear the closest scrutiny. As regards the figures I have given in the tests, Dr. Hutchinson has pointed out one or two apparent errors, but I think it is due to a misunderstanding of one or two points by him. One point which he spoke of was the impossibility of having a central station

equipped with only six horse power per car when in one of the most favorable cases he figures out eleven and a half horse power as required by a car. Without stopping to check that figure, I would say that I did not intend that six horse power in the central station would be enough if every car were in operation simultaneously at its full power, but that six horse power per car would be an average figure, some of them being at rest or going slowly, and some coming down grade, under which conditions they would assist the central station.

Another point that Dr. Hutchinson spoke of was the fact that in the table No. 1 there are eleven and a quarter kilowatts which he says had to be entirely absorbed by the motor of seven and a half kilowatts. But I call attention to the fact that the eleven and one quarter kilowatts is not all absorbed by the first motor, but a large portion of it is in the field and armature circuits of the second and third machines, and that the first machine is not overloaded. Similarly, without checking the figures up, that 97 per cent. efficiency, which he figured out, must be the result of some error.

DR. HUTCHINSON:—I beg your pardon—87—I may have said 97; I meant 87.

MR. LEONARD:—87 would be about correct, because that corresponds to the efficiency of the Edison machine of that size. The saving at the central station due to the size of apparatus of course is a factor which we can only theorize on, but I think that in view of Prof. Shepardson's curves it is rather evident that if we could supply a system in which the very large changes of load due to the starting of the car could be avoided, we could get along with less dynamo and engine capacity in the central station, and by the results of tests on traveling cranes and a few elevators, and by the figures of railway tests I am convinced that the central station capacity in engines and dynamos could be cut in two. However, I wish to call attention to the fact that the cost of the street car apparatus to-day is not such an important factor as to very materially affect the cost of the entire equipment. In one installation that I am making to-day the entire cost of the street car motors is only about 5 per cent. of the cost of the complete work, and if any very great advantage, either in economy, control or in any point might be obtained by making that 5 per cent. 10 per cent., it still would be quite an advantage. In other words, if the cost of the motors should be doubled, and we could get any decided advantage, the money would be well spent, and it would be well spent if we could even get rid of the lack of economy in the consumption of steam due to the variable cut-off incidental to rapidly changing loads that we have with the present system. Mr. Mailloux touched upon this, and it is a thing which is quite important, because it is utterly impossible to use to-day engines of the highest economy, such as are used in other practice, for the reason that the sup-

posed economy vanishes altogether in the use of compound engines, etc., because of the variable loads. The proposed system here, certainly will tend to give an almost constant load in the central station and effect a very large economy by the possibility of using engines of the highest type and operating them at the most economical cut-off.

As regards the depreciation of machinery, which I find I have made a memorandum of, there is, apparently, three times as much depreciation in three machines as in one; but when two of those machines are placed out of the influences which are the principal cause of depreciation, and when the third one is in a condition such as to have a minimum of sparking, and a minimum of heating of its field, and when the motor generator portions are also under the best conditions as regards sparking, etc., such will not be the case. The fact is, that in practice the operation of the system shows that the machinery requires almost no attention. And I know of instances in which electric elevators, which as Mr. Sprague has pointed out represent the worst kind of street car service, have been in operation for three or four months at a time without the slightest attention of any kind whatever. They have self-oiling bearings, and being entirely sparkless, they have had no attention whatever for months at a time.

The importance of a single lever rather than a multiplicity of things to handle on a street car has been touched upon both by Mr. Sprague and Mr. Mailloux in a rather forcible manner, and I call attention to the fact that in this system there would be but one lever which would accomplish all the changes of speed in either direction, and also apply the brakes, as the production of energy and the restoring of energy to the line, is the braking action for the car, and under any normal use of the system no mechanical brakes of any kind would be used, and the instance cited by Mr. Sprague of something on the track which makes reversal necessary can be dealt with by this system with the greatest of ease and the utmost rapidity.

I judge by a comment of Mr. Sprague's that he imagines that the time element in getting under way by this system is far more than it really is in practice. The fact is, that having a separately excited field for the generator, there is no cumulative action in the excitation of its field, and the change of field of the generator, and consequently the speed of the motor is extremely rapid. With the ordinary type of even cast-iron fields the action is extremely rapid.

A point which may possibly be of interest, in view of the discussion of the application of the shunt motor at high speeds on a level, is not in the paper, but it is a possibility in this method. When we have reached the full speed of operation on a level, we can avoid the necessity of running at Dr. Hutchinson's 2 per cent. efficiency by changing over from 500 volts of our generator

to the 500 volts represented by the trolley and the ground by a movement of the switch without the least sparking, and without any difficulty; because in changing over we have the electromotive force of our propelling motor as before, and that will eliminate any inefficiency of the generator, and by operating as a shunt motor, especially on the trolley system, it would enable us to make long runs with an efficiency of probably 90 per cent.

As regards the question of efficiency again, Mr. Sprague mentioned the probability of 16 amperes being required for merely the excitation of the field to accomplish the regulation. I may say in this regard that a quite recent test within the past ten days shows that the entire amount of energy required at 230 volts—that is the system in this city—that the total number of amperes required not only to excite the field but to move the elevator at its lowest speed is not 16 amperes but 8 amperes.

There are so many points of importance in the system which I called attention to in the paper which have not been touched upon, that I assume they are considered of some value. I am pleased to notice that the gentlemen agree that the objects in general are desirable even if the method used is not the best of arriving at them; but it is one way of arriving at them and at present the only way, and the efficiency of that way can only of course be determined by practice.

The use of the system upon traveling cranes has gone sufficiently far to enable us to say that the efficiency of this method is very well known. There will be in use at the World's Fair a traveling crane, perhaps the largest ever constructed, which will operate by means of a motor generator between the source of supply and the final motors as the best modification of the method, although there are several others that might have been adopted. The use of this method on passenger elevators will shortly be tested thoroughly in the case of the *Herald* Building and the United Charities Building in this city and in two or three other buildings about town though some of them are private residences where the apparatus would not be available for observation.

One of the best points in connection with the system, in my opinion, is the ability to restore energy to the line, in coming down grade or stopping, and what follows directly from that, the stopping of the car by an electric brake which is not only an electric brake in the sense of being operated by electricity but a brake which is an efficient means of stopping the car. It is the only brake I know of which will enable one to stop a car from its full speed down to a dead rest, and at all times during that period the application of the brake causes a positive saving on the system by the production of energy to assist in operating other devices. There are numerous modifications of this system, and, as you will notice, one good point is due to the fact that the secondary circuit has no connection with the line, which makes it

possible to use any voltage desired in this circuit, and if in railway practice it became desirable to use a thousand volts for our trolley, we would still be able to use upon our commutators in the propelling motors, 250 volts or 300 volts or 500 volts, or anything desired. We might make our motor such, that it would run perhaps at ten miles an hour under ordinary conditions on this system. While running at ten miles an hour we could switch over to the trolley line and continue running at ten miles an hour direct; then open our generator field, which would leave our generator with no voltage at its terminal; then we could insert it in the line between the trolley and the ground in series with the final motor, at a time when there would be no additional voltage by its introduction, and then, by raising the voltage with the generator again to 500, we have the means of securing a thousand volts on the motor armature and also securing a counter voltage of 500 to bring it to rest. A good many of these modifications will be of particular importance in certain applications. The principal point of importance, to my mind, is the flattening of the load diagram and what it accomplishes. That means, I believe, the cutting in two of the engine and dynamo, a very great increase in the economy of steam in the steam plant, especially in view of the energy restored to the line, which also assists in cutting down the capacity required in the station. This system will soon be tried on a street car. I was rather in hopes that it would be in practical operation before this time, and had it not been for unexpected delays it would have been, and it will be now probably within the next six weeks, and when I have had the system in operation practically, under expert hands certainly not prejudiced in its favor, I will be able to submit figures which are far more conclusive than those which I have assumed, and which may have errors in them. Of course the method is totally independent of the figures I have submitted, and if those figures are inaccurate in any way the result will be not affected by them.

I do not think of any other points that I wish to mention.

DR. HUTCHINSON:—I have only a word further to say. I am glad Mr. Leonard does not feel discouraged; it shows his real expectations were not high. I must call his attention, though, to the fact that he has made a mistake in the statement that a large part of the eleven and one-quarter kilowatts is supplied from the line; the only part to be deducted is the excitation of the driving motor and generator fields; that is, a total of 310 watts—practically nothing. Everything else comes from the line motor. Taking his own efficiencies this makes 21 I. H. P. in one case, and in the other. 11 I. H. P.

About efficiency in general, Mr. Leonard takes efficiencies of central station dynamos of fairly good size as a basis for the efficiency of street railway apparatus. This is no basis of comparison at all. It does not seem to me that any deduction can be made from one to the other.

Again, he did not answer the criticism that the equipment of a street car is determined not by the power for going up a five or six per cent. grade three miles an hour, but by the fact that the car must go on heavy grades at from six to nine miles per hour.

I think the plan is ingenious, and I have not the slightest doubt it will work—just as I might say storage batteries work.

THE CHAIRMAN:—I would like to call the gentlemen's attention to the fact that it is about a quarter to eleven, and as the Secretary has a little matter to bring to your attention before adjournment, it is desirable that the discussion should be as brief as possible.

MR. MAILLOUX:—I have only one question to ask Mr. Leonard. I am running cars with a storage battery which did weigh seven and a half tons, or say seven tons, and I am compelled to run them up a five per-cent. grade and carry fifty passengers, which would make the total weight not far from twelve tons. I would like to know what would be approximately the extra weight added to my car machinery, of which I already have enough on the car, by the converter—the motor dynamo of Mr. Leonard. It is evident it would have to be very much larger than that which he describes here. I should expect it to add at least as much weight as the storage battery itself.

MR. LEONARD:—I did not quite understand the question. If I understand it correctly, I would answer it by saying the method which I indicate of using a motor generator, and transmitting all of the energy to the final motor through it, would probably make it necessary to use a machine which would have a combined weight of about 120 pounds to each horse power delivered. In other words, a motor generator, at its highest speed and special design, I have no doubt could be designed and built so that the combined weight would be about 120 pounds to the horse power delivered. A motor generator would not, however, be used in the case of storage batteries.

MR. SPRAGUE:—May I ask where Mr. Leonard proposes to put the power converter? I do not want to be too inquisitive about it.

MR. LEONARD:—The question of location would be determined by the style of truck. It has been laid out to be placed between the axles of the cars, so that the axis of the motor generator would be parallel to the track.

MR. SPRAGUE:—I thought if it was in that position it would not get the care and attention which is demanded for it. If it was placed in the car body, the cost of the space sacrificed for passengers would be somewhat more than the possible saving of such a system. But practical experiments determine more than all the arguments in the world. I think I am right in my prophecy that there will not be less pounds of coal consumed per car mile run by that, than by the present system.

MR. F. V. HENSHAW:—I would like to ask why it is that the motor end of the motor dynamo transformer is so much smaller

than the rest of it—that is to say, about seven and a half K. W. against something like 20 or 21.

MR. LEONARD:—The size of the motor for the line will be determined only by the power represented by the moving load—the size of the final motor and the generator must be determined by the torque at the time when the load is heaviest, and by the volts represented by the highest speed. But if you were to run your load at a low speed comparatively, on the grades, and high speed on the levels, you would not have those two factors coincident in the first motor.

MR. T. C. MARTIN:—The other item on the programme is the report presented by the Standard Wiring Committee and revised by the Committee on Units and Standards. I would like to offer a resolution that this report be now accepted, and referred to the Congress Committee, and transferred by it to its Sub-committee on Provisional Programme for the Congress in Chicago in 1893. That, I think, will dispose of it in the only satisfactory way in which it can now be disposed of, and will give that Committee and its Sub-committee something definite to work upon. I would be glad to have the resolution seconded.

The motion was seconded by Mr. Kennelly and carried.

The meeting then adjourned.

THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, November 15th, 1892.

The 71st meeting of the Institute was held, this date, at 12 West 31st street. The meeting was called to order at 9 P. M. by Vice-President Hammer. In accordance with the notice issued, Dr. James Bowstead Williams made insulation tests of samples of various wires, using the apparatus described in his paper on "Oil versus Air as an Insulating Medium," beginning on page 601 of this volume.

The following associate members were elected at the meeting of Council:

Name.	Address.	Endorsed by
ARNOLD, CRAIG R.	Electrician and Treasurer Arnold Electric M'f'g Co., Chester, Penn.	Carl Hering. H. C. Townsend. C. O. Mailloux.
BOHM, LUDWIG K. *Ph.D.*,	Consulting Electrical & Chemical Expert, 81 Nassau St., New York City.	Carl Hering. Edwd. Caldwell. Wm. A. Rosenbaum.
DURANT, EDWARD	Electrician, with F. Pearce, 115 E. 26th St., New York City.	James Hamblet. Geo. F. Durant. R. W. Pope.
GALE, HORACE B.	Consulting Electrical and Mechanical Engineer, 40 California St., San Francisco, Cal.	E. J. Molera. W. F. C. Hasson. F. G. Cartwright.
HUNTING, FRED S.	Electrical Engineer, Fort Wayne Electric Co., Fort Wayne, Ind.	R. H. Read. A. S. Kimball. R. W. Pope.
MCELROY, JAMES F.	Mechanical Sup't, The Consolidated Car Heating Co., 131 Lake Ave., Albany, N. Y.	W. C. Miller. F. L. Woodward. R. W. Pope.
STAHL, TH.	Creusot Works, Creusot, France.	C. P. Steinmetz. S. D. Field. R. W. Pope.

Total, 7.

The following associate members were also transferred to full membership, their applications having been approved by the Board of Examiners.

BOURNE, FRANK	Electrical Engineer, Field Engineering Co., 143 Liberty St., New York City.
THOMAS, BENJ. F.	Professor of Physics, Ohio State University, Columbus, Ohio.
WURTS, ALEXANDER J.	Electrical Expert, Westinghouse Electric & M'f'g. Co., Pittsburg, Pa.
METCALFE, GEORGE R.	Electrical Engineer, Editor, *Electricity*, New York City.
STILLWELL, LEWIS B.	Electrical Engineer, Westinghouse Electric & M'f'g. Co., Pittsburg, Pa.

Total, 5.

Transactions of the Institute Already Published.

[Complete sets of the first four volumes are out of print.]

Vol. V, 1887-8.

On Electric Street Cars, with Special Reference to Methods of Grading. Anthony Reckenzaun, of London. No. 1, October, 1887.—A Collider Meter, or Instrument for Measuring the Consumption of Electricity. Elihu Thomson, Prof. Physics, Central H. S., Philadelphia. No. 1.—Recent Improvements in Apparatus for Ocean Cabling. (Illustrated.) Charles Cuttriss. Supplementary Note on the Siphon Recorder, and Ocean Telephony, by Thos. D. Lockwood. No. 2, December, 1887.—Phenomena of Retardation in the Induction Coil. (Illustrated.) William Stanley, Jr. No. 4, January, 1888.—Revision of the Patent Law. Arthur Binnott, Esq. No. 3, February, 1888.—Electric Energy from Carbon Without Heat. Willard E. Case, Auburn, N. Y. No. 4, March, 1888.—Alternating Current Electric Motors. Dr. Louis Duncan, Baltimore, Md. No. 7, April, 1888.—Maximum Efficiency of Incandescent Lamps. (Illustrated.) John W. Howell. No. 8, May, 1888.—Protection of the Human Body from Dangerous Currents. Patrick B. Delany.—The Possibilities and Limitations of Chemical Generators of Electricity. Francis B. Crocker. No. 9, June, 1888.—On Compensated Resistance Standards. (Illustrated.) Edward L. Nichols.—A New System of Alternate Current Motors and Transformers. (Illustrated.) Nikola Tesla.—Underground Electrical Conductors in Europe and America. Prof. G. W. Plympton. The Patent Court and Uniformity in Patent Office Practice. George H. Stockbridge. A Swinging Arm Galvanometer. (Illustrated.) George S. Meier. No. 10, July, 1888. The Solution of the Municipal Rapid Transit Problem. (Illustrated.) Frank J. Sprague. No. 11, August, 1888. Some objections to the Overhead Conductor for Electric Railways, M. B. Leonard. Note on Gearing for Electric Railway Motors, Almon Robinson. No. 12, September, 1888.

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[Including October, November and December, 1888.]

The Geyer-Bristol Meter for Direct and Alternating Currents. [Illustrated.] Prof. William E. Geyer. The Ardane Magnetic Call and the Ardane Intograph. (Illustrated.) E. Adam-Adamoglous. No. 1, January, 1889. Six Years Practical Experience with the Edison Chemical Meter. (Illustrated.) W. Jenkin. No. 2, February, 1889. Lightning Arresters and the Photographic Study of Self-Induction. (Illustrated.) E. G. Acheson. No. 3, March, 1889. A New System of Multiplex Telegraphy. (Illustrated.) Lieut. F. Jarvis Patten. Lightning Arresters and the Photographic Study of Self Induction. Notes by Jo. Stanford Brown and Chas. T. Child. Reply to the Notes of Messrs. Brown and Child by E. G. Acheson. No. 4, April, 1889. The Efficiency of Methods of Artificial Illumination. (Illustrated.) Edward L. Nichols. No. 5, May, 1889. Some Results with Secondary Batteries in Train Lighting. (Illustrated.) Alexander C. Brown. The Inherent Defects of Lead Storage Batteries. (Illustrated.) Dr. Louis Duncan. Electric Motor Regulation. (Illustrated.) Francis B. Crocker. Nos. 6 and 7, June and July 1889. (Double Number). Magnetism in its Relation to Induced Electro-motive Force and Current. (Illustrated.) Prof. Elihu Thomson of Lynn, Mass. On the Relation between the Initial and the Average Efficiency of Incandescent Lamps. (Illustrated.) W. H. Peirce of New York. The Efficiency of the Arc Lamp. (Illustrated.) H. Nahano, of Japan, with an introductory note by Edward L. Nichols. The Spiral Coil Voltameter. (Illustrated.) Harris J. Ryan, of Ithaca, N. Y. The Personal Error in Photometry. (Illustrated). Prof. Edward L. Nichols, of Ithaca, N. Y. Or Modern Views with Respect to Electric Currents. (Illustrated.) Prof. Henry A. Rowland, of Baltimore, Md. Classified List of Members. Nos. 8 and 9, August and September, 1889. (Double Number.) No. 10, October, 1889. Some Recent Electrical Work on the Elevated Railroads. (Illustrated.) Leo Daft, of Plainfield, N. J. Alternating Current Motors: The Evolution of a New Type. (Illustrated.) Lieut. F. Jarvis Patten, of New York. No. 11, November, 1889. Electrical Notes of a Trans-Atlantic Trip. Thomas D. Lockwood, of Boston, Mass. Some Methods of Regulating Accumulators in Electric Lighting. (Illustrated.) George B. Prescott, Jr., of New York. No. 12, December, 1889. Form and Efficiency of Incandescent Filaments. Charles J. Reed of New York. Telegraph Line Adjustments—Note on a New Gravity Cell. F. E. Delany of New York.

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Transformers. (Illustrated.) Harris J. Ryan. No. 1, January, 1890. A Review of the Modern Theories of Electricity. (Illustrated.) Prof. W. A. Anthony. No. 2, February, 1890. The Practical Working of the Electrical Subways of New York City. (Illustrated.) William Maver, Jr. No. 3, March, 1890. Some Tests on the Efficiency of Alternating Current Apparatus. (Illustrated.) Dr. Louis Duncan and W. F. C. Hasson. No. 4, April, 1890. Phenomena of Alternating Current Induction. (Illustrated.) Prof. Elihu Thomson. Electricity in the Navy. Gilbert Wilkes. No. 5, May, 1890. Electric Lighting in the Textile Industry. Wilfrid M. Fleming. Life and Efficiency of Arc Light Carbons. (Illustrated.) Louis B. Marks. Practical Aspects of the Alternating Current Theory. M. I. Pupin. Magnetic Data of the Sprague Street Car Motor. (Illustrated.) H. F. Parshall. The Industrial Utilization of the Counter Electromotive Force of Self-Induction. Thomas D. Lockwood. Nos. 6 and 7, June and July. (Double Number.) Life and Efficiency of Arc Light Carbons. Discussion. (Illustrated.) Practical Aspects of the Alternating Current Theory. Discussion. Notes on a New Photometer. (Illustrated.) Dr. Edward L. Nichols of Ithaca, N. Y. The Limitations of Steam and Electric Transportation. (Illustrated.) Oscar T. Crosby, now of New Orleans. Automatic Electric Welding Machines. Heyward Lamb, Jr., of Lynn, Mass. Nos. 8 and 9, August and September. (Double Number.) Efficiency of Transformers. (Illustrated.) Calvin Humphreys and W. H. Powell. Notes upon some Experiments with Alternating Current Apparatus. (Illustrated.) E. Prof. Harris J. Ryan. Catalogue of Membership. Revised to November 1st, 1890. No. 10 October. Preliminary Report of the Standard Wiring Table Committee. Resolutions on Adoption of American Names for Electrical Units. Interpretation of the Standard Automatic Current Apparatus. W. B. Fishenden and G. H. Wakefield. A New Method of Analyzing Armature Reactions, Applied to the Standard Arc Light Alternating Current Machine. Theodore Reid. No. 11 November. Revised Report of the Standard Wiring Table Committee. The Tendency to Corroded Wirings for Constant Potential. (Illustrated.) Dr. Louis Bell, of Hartford. A New Method of Analyzing Armature Reactions.

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